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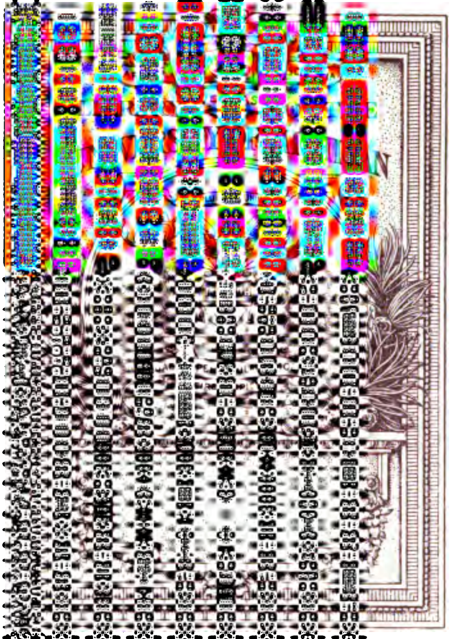

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The Astronomical Register.

No. 277. JANUARY. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The December meeting was held at the Society's apartments, Burlington House, on Friday, the 11th.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. G. L. Tupman.

Mr. James Hunter, of Edinburgh; Mr. Walter C. Johnson, 8, Vanbrugh Park, Blackheath; and Mr. Richard Wilding, Sunny Bank, Victoria Road, Preston, were balloted for, and duly elected Fellows of the Society.

Col. Tupman said that 59 presents had been received since the last meeting of the Society. Amongst them were some photographs from the brothers Henry, of the Paris Observatory. There were four photographs of stars in the Milky Way, remarkable for their sharpness and good definition. Each plate had been exposed three times, three pictures being taken, nine seconds apart, so that there were three impressions of each star, forming a little triangle of three seconds a side. It was impossible to see the separate images of the stars on the positive proofs, but it might be possible to see them on the negative. Another one is an enlarged photograph of the double star ε Lyræ. The star which is called the Debilissima is as much brighter than the faintest stars shown as ε Lyræ is brighter than the Debilissima. In fact it is quite a big star. There are many tiny stars here and there which you can just detect. The plate was exposed for two hours.

Mr. Ranyard: The smallest stars seem to correspond with the 16th magnitude.

Col. Tupman: Yes.

Mr. Ranyard: The driving is beautiful, the star discs seem perfectly round. You cannot see any defection from the circle. The instrument was constructed by Messrs. Paul and Prosper Henry. The chromatic connections must be very perfect and the driving as near perfection as possible, they deserve as much credit for making the instrument, as for the photograph, it is by a long way the most beautiful star photograph I have seen.

Col. Tupman: A few days ago there was issued from the Oxford University Observatory, a volume of observations made there containing the magnitude of some 2,800 stars, each observed many times with the wedge photometer.

A vote of thanks was accorded to the donors.

On the motion of Mr. Talmage, seconded by Mr. Lecky, Mr. Inwards, Mr. Criswick, and Mr. Buckney, were appointed auditors to examine the Treasurer's accounts.

Mr. Knobel read a paper by Mr. Neison *On the term of long period due to Mars in the expression for the longitude of the moon.* The paper referred to a criticism of a former paper on the subject by M. Gogou, and pointed out that M. Gogou had given an elaborate criticism of a short paper intended only to give the results of a provisional calculation, which was stated to need revision and extension; and further that having calculated certain additional terms which Mr. Neison had declared to be insensible, M. Gogou had only arrived at the same result.

Mr. Stone: I should be glad to ask if Mr. Neison considers the results he gave originally were correct, or whether he admits that the result M. Gogou has given is correct. The inequality with Mr. Neison's coefficient was supposed to account for certain discordances between observations and theory, which amounted to a considerable quantity. As far as I understand M. Gogou's paper is correct, but this reduces the coefficient of the inequality to about one-tenth of a second, and I do not understand from the paper whether Mr. Neison still maintains that the value he originally published is correct, or whether he now admits the correctness of M. Gogou's results.

Prof. Adams: I do not understand that Mr. Neison at all contends for the accuracy of his first published result. That made the inequality spoken of rather large, amounting to seven seconds, whereas M. Gogou finds the same inequality arising in the same way only to amount to a few hundredths of a second. I think that Mr. Neison gives up his former result, but he seems to contend here that it was quite unnecessary for M. Gogou to calculate these additional terms, because he had already declared them to be insensible, and according to M. Gogou the whole inequality, so far as he has gone in its calculation, is perfectly insensible.

Mr. Knobel: Mr. Neison states here that M. Gogou's paper is an elaborate criticism of his first provisional calculation, which only occupied three pages of the *Notices*, and which was a calculation stated at the time to be merely provisional and approximate, needing careful revision and extension.

A vote of thanks was accorded to Mr. Neison.

Mr. Ranyard read a paper "On the Connection between Photographic Action, the Brightness of the Luminous Object, and the Time of Exposure, as applied to celestial photography." Mr. Ranyard said: My experiments have been made with commercial dry plates by various makers. My object, in the first instance, was to obtain a comparison between the sensitiveness of the eye, and the sensitiveness of the photographic film for detecting small differences of illumination. With the eye a difference of $\frac{1}{60}$th in the intensity of the illumination of two adjacent fields can be detected when the illumination is not too brilliant or too faint, and when the illuminated areas are of sufficient extent. Fechner has shown that there is a physiological connection between the brightness of the illuminated fields and the difference of illumination which can be detected; he estimated that under the most favourable conditions a difference of one per cent. of illumination can be detected. In order to make such a comparison, successive exposures of equal duration may be made on adjacent parts of the same plate, and the source of light removed through a measured distance between the exposures, or if a connection can be established between the density of the photographic trace and the duration of the exposure, different parts of the plate may be exposed for different periods, without moving the source of light—a course which greatly simplifies the experiments. In order to test in a rough manner whether a light shining on a plate for a unit of time produces the same effect as a light of double the intensity shining for half a unit of time, one half a Wratten and Wainwright's "extra-sensitive plate" was exposed to the light of a candle at a distance of 5ft. for forty seconds, and the other half of the plate was exposed to the light of two similar candles at 5ft. for twenty seconds. On developing the plate, the two halves appeared to be of about the same blackness. More accurate experiments were then undertaken, and an opaque screen was placed in front of the plate, with a portion cut away, which enabled the four quarters of the plate to be successively exposed to the light at different distances and the duration of the exposure was made proportional to the square of the distance of the light. The plate was immersed in the developing solutions so that all parts were equally acted upon, and it was found that the portion exposed to a candle at a distance of 2ft. for 18 seconds was equally dense with the portion exposed to a candle at a distance of 5ft. for 112½ seconds, and similarly for other distances and exposures calculated according to the same law of inverse squares. The law appeared to hold good, whether the plates were under-developed or whether the development was pushed so as to give very dark tints. It would therefore seem that the density of the photographic trace increases regularly with the length of the exposure, and that its density is inversely proportional to the square of the distance of the source of illumination that is directly proportional to the brightness of the illumination. Plates by various makers were placed in dark slides and exposed in steps by withdrawing the shutter at intervals of seven and a half seconds. It was found that under the most favourable circumstances no difference in tint could be detected after the nineteenth step. With some of the common plates all difference of tint was lost at the sixth step, showing that the photographic sensitiveness of the plate would not register a difference between the illumination of seven and eight candles, though a distinction between the photographic trace due to an illumination corresponding to seven candles could be differentiated from the trace left by an illumination corresponding to six candles. The photographic plate is, therefore, much less sensitive than the eye for detecting small differences of illumination. During a total eclipse the corona is not usually seen till about a minute or a minute and a half before totality commences; that is, when the illumination of the atmosphere near to the sun's place is decreased to much less than 1 per cent. of the illumination of the atmosphere around the uneclipsed sun. If the corona becomes visible when the illumination of the atmosphere is reduced to a brightness of 30 times the brightness of the corona, these observations would indicate that the brightness of the illuminated atmosphere round the uneclipsed sun is at least 3,000 times as great as the brightness of the corona, and that there is no chance of photographing the corona under ordinary daylight conditions, for there is no evidence to show that the light of the corona acts much more intensely on the photographic plate than ordinary sunlight. Prof. W. H. Pickering has been making some experiments with American dry plates, and finds they will not show a difference of 5 per cent. of illumination, which confirms my results. At first I thought I could detect the position of the shutter up to some 30 steps, but on examining the plates more closely I found that there was a faint dark line across the plate wherever the shutter stopped. The dark line seemed to be due to reflection of light from the edge of the shutter; but when a little metal shade was made which threw a shadow on the edge of the shutter as it was drawn up and down the dark line could not be detected, and the utmost number of steps counted was 19.

Mr. Common: If Mr. Pickering could not photograph the moon in daylight, which has been done here, his experiments as to photographic sensitiveness of plates will not go for much.

Mr. Ranyard: I think there is a difference in the way in which these photographs have been taken here and in America. Here the moon was photographed in the focus reflector; and there it was attempted to be photographed with an ordinary photographic camera.

Captain Abney: I consider that the experiments made by Mr. Ranyard are rather a rude way of getting the relative values of exposures. It by no means follows that increase of exposure and increase of brightness are synonymous. This is a very important point, and but little attended to; but I think I may say that a double exposure is not equivalent to the double brightness of the object. In some experiments I made recently—which I hope shortly to bring before the Society—I could detect, I think, a difference of an illumination of 120 and 121, therefore I am not prepared to say that photographing the corona in daylight is impossible, more especially as I think Mr. Ranyard's theory goes upon false premises. With reference to the action of light upon the eye, I know it is very difficult to estimate very close gradations of shadow. If, for instance, you take a water spectrum—a three foot tube of water placed before the slit of a spectroscope—and examine the spectrum, apparently you get no absorption in the visible part; but if you measure it photometrically you will find there is a place in the yellow, just on the green side of D, in which there is absorption, and that the absorption is about $\frac{1}{120}$th part of the whole of the light of that part of the spectrum. It is a perfectly definite absorption, though you cannot detect it by the eye when you see the brighter parts. A photograph with proper exposure will show it at once. Therefore with the photograph you have a greater amount of sensitiveness and gradation than you have with the eye.

Mr. Ranyard: In the first part of my paper, which Captain Abney did not hear, I went rather carefully into the question whether the photographic density does increase regularly with the light, and I have a number of plates which seem to show that the density of the tint, with the same development, varies with the brightness and with the exposure. If that is the case, then the exposure for different times may be relied upon as being equivalent to exposures to different degrees of brightness. I quite agree with Captain Abney that there are differences of illumination which the eye cannot detect, but which are shown on photographs. For example, the degradation of brightness towards the sun's limb; but there the brightness is so great that the eye is dazzled and paralysed. With regard to photographing the spectrum and obtaining absorption bands, which are not seen with the eye, this may have to do with the brightness of the spectrum, or it may be due to the unequal action of light of different wave-lengths on the silver compounds or on the eye. The question is more complicated than the one I have been treating, viz., the question of the corona, where light of all wave-lengths falls together on the plate or eye.

Mr. Brett: May I venture to say, as Captain Abney used the word "shade," that the use of that word is very confusing? You do not see shade. It is the light you see.

Captain Abney: I have had the misfortune to write several books, and in an early book of mine, published by Longmans, in 1876, Mr. Ranyard will see a fairly accurate scale of shading showing the intensity of negatives with varying exposures; and he will see also that it is perfectly feasible, according to those charts, to distinguish very minute differences in shading, much smaller than the $\frac{1}{20}$th part which he seems to have arrived at as his result.

Col. Tupman: The Society has received a great many observations referring to the star shower of the 27th November. There are complete observations from Father Denza, Mr. Denning (of Bristol), Prof. Pritchard (of Oxford), Prof. Grant (of Glasgow), Dr. Copeland (of Dun Echt), Mr. Rand-Capron, Mr. Howlett, Col. Ward, and others. Naturally these papers cover very much the same ground. Most of them deal with the determination of the time of the maximum display of intensity of the meteors, and the determination of the radiant. Some of them draw comparisons with the shower as seen in 1872. Perhaps the most interesting single feature is the observation of Dr. Copeland, who succeeded by holding a prism between his eye and the meteors, with the refracting angle parallel to the direction of the motion of the meteors, and saw the spectra of seven of them. Perhaps Lord Crawford would read his paper to the meeting.

Lord Crawford: On the night of the 27th November, 1878, Dr. Copeland and myself watched at Dun Echt for the greater part of eight hours, for any possible return of the meteors of

1872. So far as the meteors in question were concerned, the result of our observation was almost entirely negative, for scarcely a meteor was seen referrable to the radiant in Andromeda. The conclusion was obvious that the meteors were not evenly distributed around the orbit of the comet. Taking the period as given by Herschel, six years and 202 days, it was evident that in 1885, November 27th, the nucleus of the comet would be only 40 days away from the earth's orbit, and this was communicated to the Society, and printed on page 520 of the 39th Volume of the *Monthly Notices*. From the 22nd to the 26th November of this year, the sky was unusually clouded at Dun Echt, so that no telescope work was possible. But on the 27th there was an occasional patch of clear sky on the horizon, and bright meteors were seen between the clouds. Between 6.13 and 6.21, Dr. Copeland counted 109 meteors, which he thought could be referred to a common source in the direction of Andromeda; all were moving slowly. They were of all degrees of brightness, from the second magnitude down to the faintest streaks. Occasionally, one occurred fully of the first magnitude, leaving a train for four or five seconds. The meteors came now singly, now in pairs, and at times five or six at a time. Dr. Copeland directed a direct vision prism towards the part where they were most numerous, and got altogether seven spectra, all similar. There was a somewhat faint spectrum with one relatively very bright line, but the position was not determined, though it was probably not far from F. The whole spectrum was like that of a bright nebula, superposed on a faint continuous spectrum. Nineteen well situated meteors observed between 7.17 and 8.3, gave a good position of the radiant point. Two assistant-astronomers remarked 700 meteors coming towards us, between 7.10 and 7.20. Dr. Copeland numbered 2,000 in the S.W., between 6.30 and 7.30. In both cases, the sky was half-clouded over. From 8 o'clock to 9 o'clock, favoured by a nearly clear sky, Dr. Copeland's nephew noted 864 meteors, again to the S.W. Shortly after 9 o'clock, the display was fast vanishing, and about 10 o'clock rain clouds settled down and concealed the sky.

Col. Tupman said that the best observations for determining the time of the maximum display, appeared to have been made at Glasgow, by Prof. Grant, and in Italy by Padre Denza. Padre Denza gave the number by four observers every fifteen minutes, from six o'clock until a little after ten o'clock. From that it would appear that the maximum was between 6.15 and 7.15, local time. It appeared to have lasted a full hour, without any appreciable change of density. The numbers actually counted in successive fifteen minutes, were 2,800, 3,100, 3,400, 4,500, 6,200, 3,500, 3,100, 3,200, 3,100, 1,700, 1,500, 1,000, 800 600, 500, 234, 312. That would fix the maximum resting on these observations, between 6.15 and 7.15—corresponding to between 5.40 and 6.40 in this country. There is 36 minutes difference in longitude from Glasgow, where they were able to keep a continuous watch. Prof. Grant had sent the numbers seen each minute by one person. He gave the numbers every consecutive five minutes, beginning at six o'clock and ending at 7·50, and they were: 54, 56, 57, 60, 80, 80, 50, 48, 70, 60, 74, 44, 22, 33, 22, 23, 24, 38, 23, 23. Those observations fixed the maximum between 6.35 and 6.40. At the rate of about 80 a minute. Add half an hour on to that for difference of longitude, and it would make the Italian time seven o'clock, so that they practically agree. As to the magnitude, nearly every one spoke of them as of the second and third magnitude, with occasionally a fire-ball, nearly all white, slow-moving meteors, many of them leaving streaks which lasted a very short time. As to the determination of the radiant points, some careful observations were made by Mr. Denning, at Bristol. He observed meteors belonging to the stream on four nights, and describes a considerable exhibition of Andromedes on Nov. 26th, counting 33, 126, 105, and 222, at different periods—a pretty rich shower. On the 27th he appears to have made most elaborate observations, and fixed the radiant area, there was no fixed centre. The radiation was from a considerable area of seven degrees in diameter, although there were one or two points in the area which seemed to be more sharply defined than others. Mr. Denning determined three radiant points on the 26th, two on the 27th, one on the 28th, and one on the 30th. He saw a considerable display of Andromedes on the 30th, but on Dec. 1st, 2nd, and 3rd he was unable to see any. The shower had absolutely passed away. There was another point Mr. Denning touched upon, which was of very great interest. The Andromedes associated with Biela's comet were all very slow moving, in consequence of the meteors overtaking the earth, so that only the difference of their velocities, or the relative velocity was observed (their relative velocity was about 12 miles per second), but Mr. Denning had observed two feeble showers, having their radiant points somewhere near the Biela radiant, indicating that other meteors also were following the earth, and that they were very swift meteors. The components of both streams he described as very swift, which was a remarkable circumstance, indicating of course hyperbolic motion. He stated that he hoped to be able to verify these streams next year.

Col. Tupman said he had observed this shower at Harrow, and determined the radiant points as carefully as he could, and he afterwards calculated the orbit, which could be done absolutely independently, by making only one assumption, viz., that the time between the showers on the 27th Nov., 1872, and the 27th Nov., 1885, corresponded to two periods of revolution. That gave the mean distance 3·483, and the orbit came out exceedingly close to that of Biela's comet. The longitude of the node was 245°·7, and Biela's was 246°·3. The longitude of the perihelion was 109°·2, and in Biela's 109°·6. The inclination was 13° 6, and Biela's, 12°·6; eccentricity, ·7508, and Biela's ·7599. The perihelion distance was ·860, and Biela's ·861; aphelion distance, 6·106, and Biela's 6·191. The periodic time of the meteors was a few days less than that of Biela's comet. Those were the main features of the papers.

Mr. Knobel: I am rather surprised at the small magnitude ascribed to these meteors. With me, the weather was not at all propitious, and although there was no first magnitude star visible, I saw a great many meteors, and it seemed to me there was a much larger proportion of meteors, brighter than first magnitude stars than described in these papers. Watching, as I was, through a dense haze of cloud, through which I was not able to see a star, I saw meteors flashing in all directions. It shows that they were very much brighter than the papers say.

Lord Crawford: I was in Gloucestershire at the time. I only saw the stars shooting for three minutes, and not more, through the haze. The only stars actually visible at the time, were those of the W in Cassiopeia, and all the meteors I saw, perhaps 40 or 50, were brighter than the stars in W in Cassiopeia. They were seen through a haze, some of them were points, evidently meteors, moving in the line of sight.

Mr. Common: I wish to draw attention to one point. I was observing the meteors with a field glass for some time, and some of the brighter ones that left streaks behind visible to the eye for two or three seconds, were visible in the field glass for nearly a minute. They had a peculiar bead-like appearance, as if they were broken up in fragments, such as you see in De la Rue's vacuum discharges, like a row of coins slightly separated. And another thing that very much surprised me, was when the sky cleared between six and seven, and when there were not many meteors visible to the naked eye, there were a good many visible with the field glass. A very large number of meteors could have been seen with the slightest optical aid that would have been otherwise invisible. I do not know whether any other person noted that peculiarity—the very long visibility after explosion. The first meteor I saw on the evening on which this display occurred, was between 5.20 and 5.25. There was a haze that entirely overcast the sky, as regards the first magnitude stars, and yet I saw behind this haze, several bright meteors flash. About a quarter to six (the sky was less clouded) they came out. From a quarter to six to seven o'clock, I counted them at the rate of 20 to 30 a minute, looking at one part of the heavens alone. I then got a little assistance, and looked to the north-east, and my assistant looking anew to the south-west. I counted from 30 to 40 a minute in the north-east. They were nearly as frequent at the south-west. That was between 6.45 and 7 o'clock. The most notable feature was the number of small meteors that were seen, and, with the aid of the telescope, the tremendous length of time that the train of the large meteors lasted. There was one about 6.45, that exploded over Cassiopeia. It was far beyond a first magnitude star, and more like Venus at her brightest.

Dr. Spitta: I saw one small meteor through my 10-inch reflector, but I could not see any train to it at all; but the train of the large meteor you spoke of about 6·45 was very brilliant, and lasted a long time.

Mr. Common: Across the W of the Cassiopeia?

Dr. Spitta: Yes.

Mr. Ranyard: I should be inclined to concur with Mr. Knobel that there were many meteors larger than first magnitude stars, some as bright as Venus. The thing that struck me most was the size of the radiant area. The trains of the meteors were rather short, for they were plunging nearly vertically into the atmosphere. But in many cases there was no great difficulty in fixing the line of flight, and mentally carrying it back to Andromeda; and it was pretty evident that the meteors were radiating, not from a point, but from an elliptic area of about 10° by 6° in diameter. It is rather difficult to explain such a radiant area connected with a flight of meteors if we suppose them to come up to the earth in different directions; but it has struck me as possible that the radiant area may be explained by the irregular shapes of the meteors, which cause them to be deflected from their original direction when they enter the earth's atmosphere. Otherwise we should have to suppose that outside the earth's atmosphere the different members of the swarm had very different orbits.

Admiral Ommanney: In the Isle of Wight I made some observations from 6·30 to 7·30, and what struck me chiefly was the aspect of the zenith, where the most brilliant display of meteors was. Immediately overhead there were almost instantaneous flashes in such quick succession that it was hardly possible to count them during the maximum display. There were two very bright meteors, which passed across Cassiopeia.

Dr. Spitta: Is any mention made in the papers of stationary meteors?

Col. Tupman: Mr. Denning has drawn a great many on his chart.

Dr. Spitta: There was a great absence of them in 1872.

Admiral Ommanney: I watched on the following night—a most brilliant night it was—and I did not see one.

Mr. Ranyard: I saw four on the following night, which seemed to come from the Andromeda radiant.

Mr. Knobel: I turned the telescope on the radiant-point, and I saw some stars glow right out. I observed up to about 9 o'clock on that evening.

Dr. Spitta: Attention was called in 1872 to the motion of the radiant-point. It commenced further away from Cassiopeia, and then extended towards it. Herschel calls a great deal of attention to this. I do not know whether any observers made a note of it this year, but the radiant area seemed to me to be quite elliptical.

Col. Tupman: On that subject I may mention that the radiant-points were determined by Padre Denza and M. Schapparelli. They both got—and they must have observed independently—an increasing R.A. and decreasing declination. Take M. Schapparelli's observations. The position of the first radiant at 6h. 35m. was 15° R.A. and + 45° declination; second radiant at 7h. 12m., 18½° R.A. + 44° declination; third radiant at 8h. 7m., R.A. 23° and declination + 42°. That was a motion of 8° in R.A. increasing, and 3° in declination. Padre Denza's observation also gave three radiants: the first at 7h. 35m. R.A. 21°, and declination + 44°; second at 8h. 20m. R.A. 26°, declination + 43°; third at 9h. 8m. R.A. 28°, declination + 42°. That was a motion of 6° in R.A., and of 2° in declination, in something less than two hours. These two sets of observations seemed to show a distinct motion of the radiant. I myself laid down the course of some 25 of the meteors which I was able to observe with accuracy, and on producing their paths backwards on a suitably drawn map, it was very evident that they did not radiate from a point, but from a distinct area of somewhat elliptic shape. (Col. Tupman illustrated this on the black-board, marking in the stars and some of the meteor-paths.) I would remind the Society that this may be explained by the theory which an anonymous writer in *The Times* advocated the other day, and which some astronomers of repute hold, viz., that the meteors were originally ejected from the earth in directions which differ from one another by a few degrees, and that consequently they come up to and intersect the earth's orbit again in similar directions. The difficulty which strikes me with regard to this theory is that if the meteors had been moving in such orbits for millions of years they would have been perturbed and unequally retarded; and if it can be supposed that they all continue to cross the earth's orbit at the same point, they would not do so at the same epoch.

Mr. Ranyard: It seems to me that the assumption that the atmosphere interferes with their direction of motion is the much more probable physical cause of the large radiant than the possibility that they were ejected from the earth. If they were ejected from the earth, one would think they would be ejected with some difference of velocity, which would give a difference of time of revolution, and they would not come up together to cross the earth's orbit. In fact after millions of years one would expect them to be drawn into very different orbits, and not to arrive together.

A vote of thanks was passed to the authors.

Mr. Knobel read a paper by the Rev. S. J. Johnson, *On an occultation of Uranus in December*, 1855. Some interest attached to this occurrence from the fact that the planet disappeared six minutes before the predicted time in March, 1871, calculated by former tables.

Mr. Knobel: I should like to call attention to the observations on the magnitude of the Nova of Andromeda, made at the Radcliffe Observatory at Oxford. I apprehend that those observations were eye-estimates, made with the telescope, and they differ very materially from the photometric determinations of the brightness of this star. I may mention the estimations at the Radcliffe Observatory of the brightness of this star, as compared with photometric observations made by Professor Pritchard at Oxford, and by Dr. Muller at Potsdam, viz.:

	At Radcliffe Observatory.	By Professor Pritchard.	By Dr. Muller.
Sept. 9	8·0	8·48	8·46
17	8·5	—	9·01
18	8·5	9·34	—
21	9·2	10·03	9·36
24	9·6	9·66	—
25	9·5	9·85	—
26	10·0	9·71	—
30	10·7	9·69	—
Oct. 12	11·2	9·81	—
13	—	—	10·04
16	11·2	9·96	—

Further observations at the Radcliffe Observatory show a steady diminution of brightness down to the 10th of December, when the magnitude was estimated at the 14th magnitude. The notes appended to the paper I can myself entirely confirm, for the Nova was only just discernible, while the nucleus of the nebula was distinctly visible.

Mr. Stone: I cannot offer any explanation of these discordances. I may say the observations were made with great care, and have been referred to one or two comparison stars, whose magnitudes are given. There is a small map with my paper, giving the magnitudes of the stars by which the magnitude of the Nova was estimated.

Mr. Knobel: They were, I suppose, direct eye-estimates?

Mr. Stone: Yes, made in the ordinary way, by comparison with stars in the neighbourhood. Two of the stars are from Argelander, and the magnitudes of the others were interpolated in the usual way.

Mr. Christie: Were precautions taken to eliminate the influence of the background of the bright nebula in these observations? Because that is a disturbing cause, both for eye-estimates and with the photometer.

Mr. Stone: There may be certain disturbances. These estimates of magnitude were referred to the stars of comparison, but the way in which the eye is affected by the bright background is not taken into account. But the question of the background is a serious question. It really comes to this: What is the correct standard of magnitude? Are you to estimate magnitudes according to the eye unarmed, or according to the eye armed with the telescope but without any extraneous absorbing medium, or is it to be the magnitude as seen through a wedge, after light has been got rid of by absorption?

Mr. Knobel: In these series of observations, there is a difference of three whole magnitudes in the eye-estimates; while with the photometer the difference is only 1½ magnitudes.

Mr. Brett: Whose photometer was it?

Mr. Knobel: Dr. Muller's Löllner photometer at Potsdam, and Professor Pritchard's wedge at Oxford.

Mr. Christie: I think the point of interest in these eye estimations is to make out how the effect of the influence on the eye of the bright portion of the nebula, immediately surrounding the star is separated from the influence on the eye of the star itself, because the danger in eye-estimates of this kind, is that the eye would perhaps estimate the total effect of the star *plus* the portion of the nebula surrounding it, and it will compare with different effect with another star at a certain distance on a dark background. In the case of the comparison star, the background would not supplement the light of the star, and therefore I should expect considerable divergence *à priori* between eye observations and photometric observations.

Mr. Common: Is it not a fact that a dark background would make the star look brighter, and a light background would make the star look less bright?

Mr. Christie: Not on the hypothesis I am considering.

Mr. Common: With a high power and decrease of brightness of the background you do see the star when you do not see it with a low power.

Mr. Christie: I do not know to which class of observations Mr. Common is referring. Under certain circumstances it may be that the eye estimates the total effect of the star *plus* the part of the background immediately surrounding it, and compares that with the total effect of another star which may have no visible background to supplement its light. But if you take photometric observations, I think the conditions may be reversed. I think both classes of observations are liable to be affected by the background, and in opposite directions.

Mr. Todd: In our estimations at Adelaide we observed the star diminish by three magnitudes, closely corresponding to Mr. Stone's result, but, of course, in Adelaide, Andromeda would be low down, and, therefore, the observations were not made under the same circumstances as those at Oxford; but the eye observations of my assistant at Adelaide closely correspond with the magnitudes assigned by Mr. Stone.

The President read a letter relating to the Elizabeth Thompson Science Fund, U.S.A., announcing that the Fund was intended to advance scientific research; that the Fund amounts to 25,000 dollars; and that application for grants should be forwarded to Dr. C. S. M——, 25, Mount Vernon Street, Boston, Mass., U.S.A.

Col. Tupman: Several Fellows present have probably lost time in sweeping for Barnard's comet. If so, they will be glad to know that the place has been sent by Dr. Copeland of Dun Echt, as follows: Time of observation, 12h. 21m. 45s. Dun Echt M.T.; R.A., 4° 12m. 29s.·07; Declination, + 5° 3m. 53s.

The following papers were also announced:—

J. E. Gore: *On a suspected new variable star in Corona Borealis.*

J. Tebbutt: *Observations of double stars at Windsor, New South Wales.*

J. E. Gore: *On the orbit of γ Coronæ Australis.*

A. Marth: *Ephemeris for the satellites of Uranus,* 1886.

A. Marth: *Addition to the Ephemeris for physical observations of Mars,* 1886.

Communicated by Lord Crawford: *Observations of Comets Brooks, Fabre, and Barnard, made at the Dun Echt Observatory with the filar micrometer of the* 15-*inch refractor.*

Prof. T. H. Safford: *Comparison of Groombridge's and Struve's right ascensions of close circumpolar stars.*

SELENOGRAPHICAL NOTES, JANUARY, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Stadius. Although this formation is ordinarily so inconspicuous that it can only be observed as a more or less complete ring under a very oblique angle of illumination, it is nevertheless fully as worthy of careful scrutiny as many which present a much more striking aspect. It is one of those objects whose details, like those of Archimedes, Fracastorius, and Plato, are not completely manifested during a cursory examination, but demand frequent and painstaking observation before the eye is able to detect all that the instrument is competent to reveal. On the evening of 15th December last, when the morning terminator nearly bisected Copernicus, though the conditions were perhaps as favourable as possible for examining Stadius and its surroundings, it was only after long gazing that the ring could be traced as a nearly entire circle. The loftiest portion of the border lies a little south of the extremity of the great mountain arm pertaining to Eratosthenes, and consists of two bright sections—according to Beer and Mädler—some 700 feet in altitude. Beyond these the wall, though insignificant in dimensions, is remarkable from the number of craters which break its continuity. Of these, the largest adjoins the bright section, and slightly to the south of it there is a short delicate cleft running through the border towards the south-west. On the extreme south there appears to be no distinct wall for some distance, but merely a slight elevation of the ground, faintly indicated by a contrast in tone between the floor and the exterior. On the south-east the border becomes once more somewhat prominent in the form of a narrow curved section, terminating in a large crater (*a*, *Neison's Map X*). It is a curious feature of this portion of the wall that there is a continuous row of small and apparently confluent craterlets at its inner base, which, with a low power, might well be taken for a cleft. Schmidt does not show these objects though they are sufficiently obvious. Immediately north of the crater *a* there is a row of large, but shallow craters, running roughly parallel to the celebrated "crater-row" of Copernicus, and terminating west of the second member, counting from the south, of this striking group. The northern border of Stadius, if the term can be applied to such an ill-defined feature, is only indicated by a just appreciable band of shade representing the slope turned away from the sunlight. Mädler and Neison show this ghostly border, but Schmidt omits it altogether, drawing Stadius in the form of a horse-shoe, open towards the north. Just outside this dim margin is situated the large crater B, the most prominent member of a group of much smaller craters, some of which form a row extending on either side of it towards the north-east and south-west, while others are clustered round it without any distinctive arrangement. On 4th March. 1849, Schmidt counted fifty craters in Stadius, but it is not quite clear whether he did not include those visible on and in the immediate neighbourhood of the border; if so, this estimate is certainly much below the mark. If, however, he referred merely to the floor, it is considerably in excess of any subsequent record. Dr. Dobie detected several in 1863; and on Jan. 6th, 1865, the late Rev. Prebendary Webb made out ten or twelve with his 5½-inch achromatic. In December, 1869, I counted ten crater-pits, with a power of 250 on my 4-inch Cooke, and on the 15th ultimo 21 craters and crater-pits were steadily seen with a power of 340 on a 8½-inch Calver. The largest of these—nearly equal in size to the crater B, near the north wall—is situated on the south-west quarter of the floor, not far from the border; and between it and the latter there is a pair of crater-pits, while beyond them, running up to the border, are three small confluent craters, with a fourth in a line with them just on the opposite side of it. Most of the objects on the floor of Stadius are crater-pits, and in two or three instances they are double, the smallest ranking in size and visibility with the larger crater-cones in Plato. Schmidt's map shows 16 craters and crater-pits in the interior, only four or five of which agree in position with those recorded by me. The "crater-row" of Copernicus is so closely associated with the north-eastern flank of Stadius and its string of craters that it seems probable that they represent a common axis of eruption, there being only a very short break in the chain between the southern end of the row and the largest crater forming the northern termination of this section of the border, and the general direction of the two being the same. The individual craters composing the "crater-row," however, are much larger than any on the border, and will repay careful examination with high powers. Their most noteworthy characteristic is extreme irregularity of shape. Some are oval, others spindle-shaped, one pair confluent, like an hour-glass; while the walls, only roughly corresponding to these various forms, are manifestly of very variable width and stand up considerably above the general level of the neighbouring country. The rugged region immediately east of the row is plentifully dotted with minute craters and crater-pits, resembling in this respect that north-west of Gambart; and a short distance to the west of the row is a chain of four crater-cones, running from north to south. They stand near the eastern limits of the dark area *k*, shown in *Neison's Map X.* The interior of Stadius, under a low sun, is nearly uniform in colour. On the 16th December the darkest portion of the floor was that immediately adjoining the western border. Here there was a well-marked dusky band, running nearly parallel to the border. With this exception, there were no very great differences in the tone of the floor, though perhaps the south-eastern quadrant was rather darker than the remainder.

Kempston, Beds: 21 December, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

FORTUITOUS ACCORDANCE OF TRANSIT RESULTS.

The following is, I think, a good instance of the sort of fortuitous accordance of results which is so often mistaken for evidence of accuracy of observation.

I have erected a stand for a small transit instrument on the lawn here, consisting of an inverted tree trunk sunk about 6 feet in the ground, and carrying an oak plank screwed upon the top, the whole tarred or painted. The transit is an old one of Dollond's, with an object-glass of 1·0 inch, and focal length 15 inches, mounted on a brass stand, of which the Y's are on tripods cast in one piece with the base, and 10½ inches between bearings. The diaphragm consists of a thin glass plate with five lines traced on it by myself. This much will suffice to give an idea of the capabilities of the instrument.

I had adjusted the stand approximately in the meridian, and levelled the Y's when I last used the transit, some six months ago, since which time, for various reasons, I had not taken any observations, and there was every chance that the adjustments had been disturbed.

On the 17th inst., wishing to get a rough estimate of the error of my chronometer (which had been compared with a good clock, by Shelton, *daily*, all through the six months) preparatory to resuming more regular observations, I decided to take one or two transits without attending to adjustment. As a fact, I took four, at widely different zenith distances. As the wind was high and troublesome, I only noted the times to entire seconds, and also was not particular about getting all five times. The following were the four stars, their z. d., the number of wires observed, and the concluded chronometer errors:—

1	α Persei	+ 1°	35′	5	—8m.	41·0
2	γ Eridani	+ 64	52	1	8	67·4
3	A Tauri	+ 29	16	4	8	55·4
4	α Tauri	+ 34	46	3	8	57·2

The decimal of the apparent error is due to the fact that each observed time was separately reduced to centre wire, and also because it is fairly permissible, where there are four or five such reduced observations, to retain the first decimal of the mean.

These four results were far too discordant to be regarded as merely bad observations; besides, the reductions to centre wire had proved the contrary. So I computed an empirical deviation factor, and on applying it obtained the following corrected results, viz.:—

1	α Persei	Chr. Err.	—8m.	39·7s.
2	γ Eridani	„	8	39·7
3	A Tauri	„	8	39·8
4	α Tauri	„	8	39·6

Not having revised the work, I may, of course, have made a mistake in reducing. That, however, would not affect the noteworthiness of the curious accordance, which, as it strikes me, should make one cautious of suspecting cookery in similar cases.

Collingwood: Nov. 23. J. HERSCHEL.

GALILEO'S RANK IN THE MOON.

Sir,—Every one to whom it has occurred must have been struck with the fact that, in the naming of spots in the moon, Galileo has had allotted to him so insignificant a representative that omission itself would scarcely have been more marked as a slight upon his name. As this cannot be supposed to be accidental (and if it were, the fact of his degradation remains), I think most astronomers would agree in regretting—for on no other ground can one account for it—that his frailty should be visited upon him so severely. Some such spot as Bullialdus would have been the least that was Galileo's due.

At a time, therefore, when the moon is undergoing additions to her nomenclature, ought not this inconsistency to be righted before all the remaining objects of any prominence are appropriated to less historic names?

There is a large area into which Capuanus projects, having Mercator and Campanus on the N., and Ramsden just within its S.E. border, and whose boundary is completed by a crater S.W. of Ramsden, marked "f" by Neisson, the banks of Capuanus, the elevated objects connecting the S. side of Capuanus with Cichus, and the hills connecting Cichus with Mercator. This might very well be called *Mare Galilei.* That it is distinctly a small sea, the telescope will readily show; also it certainly wants naming.

EDWARD ALLEN, M.A., F.R.A.S.

Castlechurch Vicarage: Nov. 25, 1885.

THE METEORS OF NOVEMBER 27th, 1885.

As seen at East Tisted Rectory, Alton, Hants.

A few hours in anticipation of the time indicated in the notice so opportunely published by Dr. Copeland, of Lord Crawford's Observatory, at Dun Echt, the expected flight of meteors so mysteriously connected with the orbit of the vanished comet of Biela, was seen here, for a limited time, in great magnificence from about 6h. to 7h. 15m. p.m., on Friday evening. They appeared in such multitudes, that it was hopeless for any single observer to attempt to count them—radiating from Gamma Andromedæ, as was predicted, to all parts of the heavens; but chiefly, as it seemed to me, affecting the regions contiguous to Pegasus, Cygnus and Hercules; though this idea may have been only illusory, and dependent on the fact that the sky was generally the clearest in those quarters. I did not see the celebrated meteor shower of November 27th, 1872, but I should certainly say that those of Friday last though, perhaps, not so brilliant in general as the *Leonids* of November 13th and 14th, 1866, yet were decidedly far more numerous. For whilst with the aid of one assistant, I was enabled to estimate the richest part of the shower of these last at about 200 per minute, for the eight minutes only, from 1h. 2m. to 1h. 10m. a.m., November

14th (see Report of "British Association" for 1867, p. 390). I should say that on Friday last, from 6h. to 7h. p.m., they were relatively about 250 per minute, almost continuously. A few that burst into sight close to Gamma Andromedæ appeared, as of course would necessarily be the case, almost if not quite stationary—the meteors at that point travelling directly in the line of vision. All were of a whitish colour, and many left trains behind them for a few seconds, of a somewhat ruddy or greenish hue. Unfortunately the sky was completely covered with clouds and mist by about 7h. 15m. p.m.; though at about 8h. 15h., I could still perceive now and then, between the vapours, that the glorious pageant was still proceeding.

F. HOWLETT.

ACCOUNT OF THE TOTAL ECLIPSE OF THE SUN OF SEPTEMBER 9th, 1885,

As seen by Lieutenant Herbert Russell, late of H.M. 8th Regiment, near Palmerston, Northern Island, New Zealand.

We had a magnificent view this morning of the total eclipse of the sun. The totality lasted one minute-and-a-half, when the corona was visible in all its glory. It was one of the grandest, sublimest sights I ever saw. There were some beautiful dark pink flames (or rather small clouds) floating on either side of the sun, besides other flames of a white colour. I was looking through the telescope given me by my brother G——, but they were just as easily seen by the naked eye. It was not *quite* dark, as the corona was so brilliant that it gave a certain amount of light; but many of the stars were distinctly visible, especially one quite close to the sun. There was hardly a cloud in the sky. I would not have missed it for anything. It was about 8 a.m.

NOVEMBER METEORS.

In *La Nazione*, of Nov. 30th, Padre Denza gives a long account of the meteors of Nov. 27th. From 6 to 6¼, 2,800 were counted; from 10 to 10h. 8m., 312. The maximum was from 6¾ to 7¼. From 7 to 7¼, 6,200 were counted. The total number observed was 39,346; but, if estimated on the supposition of a constant clear sky, and four observers, the number would amount to 60,300. In 1872 33,000 were seen in 6 hours. This time, with less favourable weather, 39,000 were counted in 4 hours. At the maximum the spectacle was altogether imposing, and difficult to describe. From every quarter of the firmament it rained masses of stars, which were mostly followed by bright streaks. Very many exceeded the first magnitude, and not a few resembled bolides. With few exceptions the meteors radiated from the usual point, not far from γ Andromedæ.

From telegrams from all parts of Italy, and some from Sicily, we gather that the phenomenon was equally well seen.

A contadino (peasant) on the property of Conte Baldelli, near Florence, reported the spectacle to his mistress in the words, *Nevicava stelle!* (It was snowing stars!)

REVIEWS.

Copernicus. In Nos. 35 and 36 of this journal, which is discontinued for the present, is a long paper by Ralph Copeland, Ph. D., an account of some recent astronomical experiments at high elevations in the Andes, which it is hoped may be of some use in pointing out the possibilities of a thoroughly planned astronomical expedition to the Titicaca table lands. The astronomer can reckon on nearly nine months of good weather, or sufficiently good to furnish abundant astronomical occupation. Dr. Copeland found, as was to be expected, great diminutions in the refractions, which leads him to believe that the time will eventually come when an improved transit-circle will be used at a high level in the fundamental branches of practical astronomy, such as the formation of a standard star catalogue, the determination of the obliquity of the ecliptic, and, above all, in finding the absolute parallaxes of the fixed stars. He thinks that possibly it may be found that the summit of one of the numerous islands in the great freshwater lake (Titicaca), will afford advantages in the way of greater approach to uniformity of temperature, freedom from dust, and an absence of local disturbance in the air that cannot be found on the mainland. Such a station, with the barometer at a nearly uniform height of 18·7 in., and the thermometer between the absolute *extremes* of 70° and 32°, seems almost the ideal site for a permanent observatory. The steamers that ply on the lake would furnish the necessary transport, while native balsas, or rush-boats, would not be slow in providing most of the ordinary necessaries of life. The lake district seems to be in a great measure exempt from earthquake shocks. The plains between La Paz and the lake offer the choice of all heights between 12,500 and 13,300 feet, with a remarkably free horizon, and the possibility of communicating with La Paz. Two gentlemen passed several days and nights on the snowless summit of the Misti volcano, without serious inconvenience, although the height of 18,650 feet is necessarily associated with so low a barometer as 14½ in. "I need not here dwell," says Dr. Copeland, "on the extreme interest attaching to measures, at such an elevation, of the radiant heat emitted from the sun, the more so as such determinations could probably be made at all zenith distances, down to little short of 92°, nor would such observations be without their great value at the regular station lower down.

"As to the great facilities of reaching this very remarkable or, we may say, unique region, they are unrivalled. At the seaport of Mollendo, the heaviest cases or castings can be transferred by large launches to the railway pier, where they can be lifted immediately on to the railway trucks, and so carried to any part of the line up to 14,666 feet. If it is preferred to go forward into Bolivia, the railway train will hand the packages directly over to the steamers on lake Titicaca; and only on the southern end of that inland sea has land carriage by ordinary roads to be resorted to, but even then loads up to two tons in weight can be efficiently dealt with." In conclusion, Dr. Copeland puts on record his extreme indebtedness to the cordial liberality of Lord Crawford, which alone rendered his interesting journey possible.

In a paper in the same number, by the Rev. S. J. Perry, *On the chromosphere in* 1883, mention is made of a prominence, on the 30th of October, that reached the unprecedented height of almost 5′, or about one-sixth of the solar diameter. This enormous prominence appeared in the shape of a cloud, stretching from 70° 30′ W. of N. to 79° 40′, along the sun's limb. On the 16th of May the whole chromosphere was at an unusually high level, the mean height being 9″·8, the mean for the whole year being 8″·27.

The following paper is by David P. Todd, M.A., being *A comparison of Leverrier's tables of Uranus and Neptune with those by Newcomb, of the same planets.* At the time of publication of Leverrier's tables of Uranus the planet was farther from his positions than from Newcomb's; and recent observations show that Uranus is now about midway between the two, the corrections to Leverrier's longitudes being minus, and to Newcomb's plus. During the last decade of the present century, however, Leverrier's tables appear likely to be somewhat more accurate than Newcomb's.

Leverrier's tables of Neptune being founded on nine years' more observations than Newcomb's, the planet is now following the former tables much more accurately than the latter. The present correction to Leverrier's heliocentric longitudes is about 0″·5, while the planet is about 2″·5 in advance of Newcomb's positions. It seems likely that, by the end of the present century, Newcomb's tables will be largely in error, probably in the neighbourhood of 10″.

DUN ECHT CIRCULAR, No. 100.

Professor Krueger announces from Kiel the discovery of a faint comet at Paris:

1885.	Greenwich M. T.	R. A.	Decl.
Dec. 1.	9h. 25m. 3s.	0h. 39m. 8s·5.	N. 21° 2′ 25″.

Daily motion in right ascension, *minus* 37′.
,, ,, in declination, *minus* 3′.

Lord Crawford's Observatory, RALPH COPELAND.
Dun Echt: 1885, December 3.

DUN ECHT CIRCULAR, No. 101.

The announcement has been received by Harvard College Observatory, from Dr. Lewis Swift, Director of the Warner Observatory, of the discovery of a comet by Barnard.

1885.	Greenwich M. T.	R. A.	Decl.
Dec. 3.	15h. 7m·2.	4h. 21m·9.	N. 4° 45′.

Daily motion 35′ towards the north preceding.

The above message was forwarded by Professor Krueger, of Kiel.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, December 5.

DUN ECHT CIRCULAR, No. 102.

Elements and Ephemeris of Comet "Fabry."

The orbit is computed from observations on December 1, 4, and 7, and leaves errors of —8″ and —2″ in the long. and lat. of the middle place.

Elements.

T = 1886, March 9·7944 Berlin M. T.
$\pi - \Omega$ = 132° 36′ 19″ }
Ω = 32 17 32 } Mean Equinox 1885·0.
i = 47 18 0 }
$\log q$ = 9·69654

Ephemeris for Berlin, Midnight.

1885.	R. A. h.	m.	s.	Decl. °	′	log. Δ	log. r	I
Dec. 12	0	14	22	N. 20	46·5	0·0827	0·2549	1·2
13	0	12	24	20	45·4			
14	0	10	29	20	44·5			
15	0	8	37	20	43·8			
16	0	6	47	20	43·3	0·0831	0·2397	1·3
17	0	5	0	20	43·0			
18	0	3	16	20	42·8			
19	0	1	35	20	42·9			
20	23	59	57	20	43·1	0·0837	0·2236	1·4
21	23	58	22	20	43·6			
22	23	56	49	20	44·2			
23	23	55	19	20	45·1			
24	23	53	52	20	46·1	0·0844	0·2068	1·5
25	23	52	27	20	47·4			
26	23	51	5	20	48·9			
27	23	49	46	20	50·6			
28	23	48	29	20	52·6	0·0849	0·1892	1·6
29	23	47	14	20	54·8			
30	23	46	2	20	57·3			
31	23	44	52	21	0·0			
'86 Jan. 1	23	43	45	21	3·0	0·0850	0·1706	1·8

The brightness "I" on December 1 is taken as unity.
Dr. H. OPPENHEIM.

Berlin, 1885, December 9.

Lord Crawford's Observatory,
Dun Echt: 1885, December 14.

DUN ECHT CIRCULAR, No. 103.

Elements and Ephemeris of Comet "Barnard."

The elements are derived from a Berlin observation of December 5, and from two others made at Dresden on the 7th and 10th inst. The outstanding difference of the middle observation are +4″ and —3″.

Elements.

T = 1886, May 14·1430 Berlin M. T.
$\pi - \Omega$ = 116° 31′ 57″ ⎫
Ω = 66 22 12 ⎬ Mean Equinox 1885·0
i = 94 8 7 ⎭
log q = 9·74184

Ephemeris for Berlin Midnight.

1885.	R. A. h.	m.	s.	Decl. °	′	log Δ.	I
Dec. 12	4	0	12	N. 5	33·4	0·2506	1·1
13	3	57	41	5	39·8		
14	3	55	10	5	46·4		
15	3	52	39	5	53·2		
16	3	50	7	6	0·3	0·2444	1·2
17	3	47	35	6	7·5		
18	3	45	4	6	14·9		
19	3	42	33	6	22·5		
20	3	40	3	6	30·2	0·2400	1·3
21	3	37	33	6	38·1		
22	3	35	3	6	46·1		
23	3	32	34	6	54·3		
24	3	30	5	7	2·6	0·2372	1·3
25	3	27	38	7	11·1		
26	3	25	12	7	19·8		
27	3	22	47	7	28·6		
28	3	20	23	7	37·5	0·2358	1·4
29	3	18	1	7	46·6		
30	3	15	39	7	55·8		
31	3	13	19	8	5·1		
'86 Jan. 1	3	11	1	8	14·6	0·2359	1·5

The brightness "I" on December 5 has been taken as unity.

Dr. H. OPPENHEIM.

Berlin, 1885, December 10.

Lord Crawford's Observatory,
Dun Echt: 1885, December 15.

DUN ECHT CIRCULAR, No. 104.

Mr. J. E. Gore, of Beltra, Ballysodare, Ireland, writes that at 9h. 20m. p.m., on the 13th, observing with a binocular, he found a reddish star about 6 magnitude and about 20′ following χ' (54) Orionis. Not finding it given by Harding, Lalande, Heis, or Birmingham, Mr. Gore suspects that it is either a "new star" or a remarkable variable.

Last night the undersigned found it of the 6½ magnitude and of an orange red colour. It has a very beautiful banded spectrum of the 3rd type, 7 dark bands being readily distinguished with the prism. The bright intervals seem full of bright lines, especially in the green and blue.

The mean place for 1885 is:—5h. 48m. 59s. + 20° 9′·4.

It is not in the Bonn maps.

RALPH COPELAND.
L. BECKER.

Lord Crawford's Observatory,
Dun Echt: 1885, December 17.

THE PLANETS FOR JANUARY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	17 17 52	S. 20 30½	7″·6	22 29·9
	9th	17 46 27	S. 22 0	6″·4	22 27·0
	17th	18 28 46	S. 23 3½	5″·6	22 37·7
	25th	19 17 33	S. 23 3	5″·2	22 54·9
Venus ...	1st	21 53 20	S. 13 9½	33″·4	3 8·6
	9th	22 13 4	S. 10 5½	37″·4	2 56·7
	17th	22 26 33	S. 7 13½	42″·4	2 38·8
	25th	22 32 16	S. 4 49	48″·2	2 13·0
Mars ...	1st	11 35 42	N. 5 49½	10″·6	16 48·7
	9th	11 42 15	N. 5 21	11″·4	16 23·8
	17th	11 46 32	N. 5 8	12″·2	15 56·6
	25th	11 48 13	N. 5 11½	13″·2	15 26·8
Jupiter ...	1st	12 22 14	S. 0 58	35″·2	17 35·1
	9th	12 23 35	S. 1 4½	36″·0	17 5·0
	17th	12 24 13	S. 1 6	37″·0	16 34·2
	25th	12 24 6	S. 1 3	37″·8	16 2·6
Saturn ...	1st	6 18 58	N. 22 32	18″·6	11 32·8
	9th	6 16 13	N. 22 34½	18″·4	10 58·6
	17th	6 13 39	N. 22 36½	18″·4	10 24·6
	25th	6 11 21	N. 22 38	18″·2	9 50·9
Neptune ...	1st	3 24 27	N. 16 50		8 38·8
	17th	3 23 37	N. 16 47½		7 35·0

Mercury rises an hour and three-quarters before the sun at the beginning of the month, the interval gradually decreasing.

Venus sets shortly after 8 p.m. on the 1st, the interval decreasing to 7.30 p.m. on the last day.

Mars rises an hour and three-quarters before midnight on the 1st, the interval increasing.

Jupiter rises half an hour before midnight at the beginning of the month, the interval increasing.

Saturn sets about twenty minutes before the sun on the 1st, and afterwards earlier each night.

GALILEO'S OBSERVATORY.

We find from a Florence paper, Nov. 25th, that the *Torre del Gallo*, the ancient observatory of Galileo, is to be let. The villa is furnished with every comfort for the winter season.

Books received:—A Popular History of Astronomy. By Agnes M. Clerke. Edinburgh: Messrs. Adam & Charles Black. 1885.—Annual Report of the Smithsonian Institution for 1883.—Astronomische Nachrichten. No. 2692.—L'Astronomie. Par Camille Flammarion. No. 12. Paris: Gauthier Villars. 1885.—Bulletin Astronomique. Par M. T. Tisserand. Paris: Gauthier Villars. 1885.—Sirius. Leipsic, 1885.—The Sidereal Messenger, Carleton Coll. Minn.

ASTRONOMICAL OCCURRENCES FOR JANUARY, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Sirius.
Fri	1		Sidereal Time at Mean Noon 18h. 44m. 14·71s.	3rd Tr. E. 2nd Sh. I.	12 10 19 14	11 53·9
Sat	2	19	Conjunction of Moon and Mercury 2° 34′ S. Sun's Meridian Passage 4m. 20·96s. after Mean Noon			11 50·0
Sun	3		Saturn's Ring: Major axis=46″·58 Minor axis=20″·51	4th Ec. R. 2nd Ec. D. 2nd Oc. R.	13 13 11 13 29 22 18 44	11 46·1
Mon	4	19 43	● New Moon	1st Sh. I. 3rd Ec. D. 1st Tr. I.	18 10 18 29 20 19 23	11 42·1
Tues	5			2nd Tr. E. 1st Ec. D. 1st Oc. R.	13 46 15 31 50 18 58	11 38·2
Wed	6			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	12 38 13 51 14 55 16 6	11 34·3
Thur	7			1st Oc. R.	13 26	11 30·3
Fri	8	18	Conjunction of Moon and Venus 0° 38′ S.	3rd Sh. E. 3rd Tr. I. 3rd Tr. E.	11 30 13 11 16 0	11 26·4
Sat	9					11 22·5
Sun	10	0	Conjunction of Saturn and μ Geminorum 0° 0′	2nd Ec. D.	16 4 21	11 18·5
Mon	11			4th Sh. I. 1st Sh. I.	17 39 20 3	11 14·6
Tues	12			2nd Tr. I. 2nd Sh. E. 2nd Tr. E. 1st Ec. D.	13 29 13 54 16 15 17 24 49	11 10·7
Wed	13	0 24	☽ Moon's First Quarter Sidereal Time at Mean Noon 19h. 31m. 33·39s.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	14 31 15 42 16 43 17 59	11 6·7
Thur	14	7 9 8 11	Occultation of B.A.C. 830 (6) Reappearance of ditto Sun's Meridian Passage 9m. 22·78s. after Mean Noon	1st Ec. D. 1st Oc. R.	11 53 3 15 16	11 2·8
Fri	15		Illuminated portion of disc of Venus=0·253 Illuminated portion of disc of Mars=0·931	3rd Sh. I. 1st Tr. E. 3rd Sh. E. 1st Tr. I.	12 9 12 25 15 17 16 59	10 58·9
Sat	16	3 56 4 58 3 58 4 56 4 33 4 58 5 57	Occultation of θ¹ Tauri (4½) Reappearance of ditto Occultation of θ² Tauri (4½) Reappearance of ditto Near approach of 75 Tauri Occultation of B.A.C. 1391 (6) Reappearance of ditto			10 54·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.	(Jan. 16th continued.)		h. m. s.	h. m. Sirius. —
Sat	16	5 5 5 20 5 55 7 48 8 49	Near approach of 80 Tauri (6) Near approach of 81 Tauri (5½) Near approach of 85 Tauri (5) Occultation of α Tauri (1) Reappearance of ditto			
Sun	17	3 38 4 28 12 54 13 59	Occultation of 117 Tauri (6) Reappearance of ditto Occultation of 130 Tauri (6) Reappearance of ditto	2nd Ec. D.	18 39 50	10 51·0
Mon	18	6 37 9 44 1	Occultation of 16 Geminorum (5½) Reappearance of ditto Conjunction of Moon and Saturn 4° 8′ N.			10 47·1
Tues	19	19 44 14 3 19 49	○ Full Moon Occultation of 1 Cancri (6) Reappearance of ditto	2nd Sh. I. 2nd Tr. I. 2nd Sh. E. 2nd Tr. E.	13 37 15 56 16 27 18 41	10 43·1
Wed	20			1st Sh. I. 1st Tr. I. 1st Sh. E.	16 25 17 32 18 42	10 39·2
Thur	21			2nd Oc. R. 1st Ec. D. 1st Oc. R.	12 58 13 46 3 17 6	10 35·3
Fri	22	8 7	Occultation reappearance of 17 Sextantis (6)	1st Tr. I. 1st Sh. E. 1st Tr. E. 1st Sh. I.	12 0 13 10 14 15 16 7	10 31·3
Sat	23	15 40 16 40 14	Occultation of B.A.C. 4043 (6½) Reappearance of ditto Conjunction of Moon and Mars 2° 59′ N. Saturn's Ring: Major axis=45″·94 Minor axis=20″·41	1st Oc. R.	11 33	10 27·4
Sun	24	6	Conjunction of Moon and Jupiter 0° 17′ S.			10 23·5
Mon	25					10 19·5
Tues	26	13 31	☾ Moon's Last Quarter	3rd Oc. R. 2nd Sh. I. 2nd Tr. I.	13 27 16 10 18 21	10 15·6
Wed	27			2nd Sh. I.	18 18	10 11·7
Thur	28			4th Sh. I. 4th Sh. E. 2nd Oc. R. 1st Ec. D.	11 39 14 21 15 23 15 39 5	10 7·7
Fri	29			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	12 47 13 49 15 4 16 4	10 3·8
Sat	30			1st Oc. R.	13 21	9 59·9
Sun	31			1st Tr. E.	10 30	9 55·9

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Jan. 1	−47	42 E.	Jan. 11	+10	44 M.	Jan. 21	+69	8 E.
2	59	52	12	− 1	26	22	56	59
3	72	2	13	13	35	23	44	49
4	84	11	14	25	45	24	32	40
5	+83	40 M.	15	37	54	25	20	30
6	71	31	16	50	4	26	8	20
7	59	21	17	62	14	27	− 3	49
8	47	11	18	74	23	28	15	59
9	35	2	19	86	33	29	28	8
10	22	52	20	+81	18 E.	30	40	18
						31	52	28

Moon farthest from the Earth, Jan. 6, 21h.
,, nearest the ,, ,, 20, 1h.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1885.

Common, A. A.
Perigal, H.
Rogerson, G. R.
Warriner, H.

To Dec., 1886.

Ellis, W. M.
Fielding, G. E.
Johnson, Rev. S. J.
Joynson, J.
Klein, S. V.
Knott, G.
Numsen, W. H.
Walker, G. J.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, Chief Office, London.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription postage included.

The pages of the *Astronomical Register* are open to all suitable communications Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 278. FEBRUARY. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The January meeting was held at the Society's rooms on Friday, the 8th, 1886.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. G. L. Tupman.

Mr. J. E. Evans, of the Training College, Dindigal, Madras.
Mr. George Ford, B.A., of Christ's College, Cambridge.
Mr. Jacob Gerhard Lohse, of 68, Falsgrave Road, Scarborough.
Mr. William Irving Page, of Wimbledon Common.
Captain George Charles Parker, of H.M.'s Indian Marine, Kurrachee, India.
Mr. Julian Trippling, of Belle Vue, Heathfield Gardens, Chiswick, W., and
Mons. E. L. Trouvelot, of the Observatoire de Meudon, Seine-et-Oise, France,

were balloted for, and duly elected Fellows of the Society.

Col. Tupman read the minutes of the previous meeting, which were confirmed.

Col. Tupman said that 54 presents had been received since the last meeting, amongst them a batch of Photographs from Paris, well deserving attention, and the thanks of the Society were voted to the senders.

Captain Noble asked if the Secretary could inform him if Dr. Collett, of Leicester, was a Fellow of the Society. A person of that name was advertised in a Sussex newspaper as "M.D., F.R.A.S." He was quoted as giving testimonials to a quack medicine.

Mr. Knobel said he had better announce officially that there was no name of Collett in the Society's list for June, 1885.

Mr. Talmage said that sometime ago a man who came into his neighbourhood to lecture, appended the letters F.R.A.S. to his name. He wrote to this man, and told him that on the night of his lecture he should attend the meeting and announce that he was a fraudulent individual. The lecturer replied at once, and said he was extremely sorry for the mistake, which he believed arose with the printer. He (Mr. Talmage) replied that he was willing to accept his explanation, but that he should watch the man's advertisements, and if he saw F.R.A.S. at the end of his name, he should follow him all over England, and appear wherever he appeared, and denounce him as a fraudulent impostor. Since then he had not heard anything about him.

Captain Noble said that he was not going to follow this Dr. Collett all over England (laughter), but he thought that five shillings would be well expended in an advertisement in the county paper, saying that Mr. Collett, of Leicester, had no claim whatever to the title F.R.A.S.

Mr. Knobel said that it is a question which would have to be considered seriously. If it is the wish of the Fellows of the Society that the Secretary should insert such an advertisement, it could be done; but I think there should be a definite motion before us upon the subject. I remember an instance of a gentleman lecturing at Leamington, who announced himself as F.R.A.S., and the local papers denied that he had any right to use the letters, so he appealed to me, and I wrote to say that the gentleman had the right to use the letters, as he had complied with every regulation, and the bye-laws of the Society.

After some further discussion:

Captain Noble proposed: "That an advertisement having appeared in several papers containing testimonies to the virtues of an American oil, by a person described as —— Collett, M.D., F.R.A.S., and as no person of that name is, nor ever has been, a Fellow of the Royal Astronomical Society, the Secretary be instructed to take such steps as may be necessary to proclaim the fact, that Mr. Collett has no connection whatever with the Society."

Admiral Sir Erasmus Ommanney seconded the motion, which was agreed to.

Dr. Copeland, Mr. Leeson Prince, Mr. Lister Kaye, Mr. Thomas Wm. Backhouse, Mr. Henry Pratt, Major Fellowes, Mr. W. S.

Franks, and Mr. G. H. With, were formally admitted Fellows of the Society, Mr. Ranyard acting as proxy on their behalf.

Mr. Ranyard said: I should like to say that I do not think it at all necessary that this form should be gone through before Fellows of the society can vote and exercise their right as Fellows. These gentlemen signed a notice with regard to the election of the new council, and some doubt has been raised as to——

Mr. Knobel: I rise to order; I do not think that any remarks on this point are in order.

The President ruled that Mr. Ranyard was out of order in speaking on a subject that was not before the meeting.

Captain Noble said that if half-a-dozen Fellows could be admitted as they had been that evening by proxy, he did not see the necessity of their being admitted at all before they could vote. The question should not be burked. It was a matter of considerable importance whether Fellows who had not gone through the empty form of being admitted by proxy could vote with respect to the management of the Society.

Mr. Roberts read a paper entitled *Photographic Maps of the Stars.* He said a letter from Admiral Mouchez, published in the *Monthly Notices* for November, shows the necessity of making known what has been already done in the direction of Stellar Photography. In 1883, I made experiments in photographing stars with ordinary portrait lenses, varying between 3-8ths of an inch to 5-inches in aperture; the lenses being made by English, French, and American opticians. One by Lerebour, of 2-inches aperture, gave the best results, and I adopted it as a standard for comparing the others. The comparisons were made by attaching 2 cameras to the declination axis to my 7-inch equatorially mounted refractor, by Cook, and taking some photographs of some well known groups of stars under precisely the same conditions. The plates were selected with regard to uniformity; and sometimes a plate would be cut in two, a half being exposed in each camera under similar conditions. The result of these experiments was that I adopted a silver-on-glass reflector of 20-inches aperture for my photographic telescope. The suggestion of Admiral Mouchez to allow an exposure of from one to three hours would make the progress of the work extremely slow. 15 minutes' exposure in a clear sky can often be made, but an exposure of three hours' duration would be rarely possible. Stars of small magnitude can be photographed by my reflector in 15 minutes, but it would be misleading to compare photographic magnitudes with those which have been determined by eye observations, until closer comparisons have been made between a large number of stars, and taking into account errors in focussing, variable sensitiveness of plates, duration of exposure, varying actinism, moisture, density of image in development, &c.; these should all be eliminated or reduced to a constant factor. In illustration of this, I have placed discs on the board, showing the result of photographing Polaris between September 15th and December 15th. Seventeen photographs have been taken with varying exposures, from 1 second to 32 minutes. On some of them the *companion* was photographed faintly in 4 seconds, while on others, with longer exposures, it does not appear. With an exposure of 32 minutes the disc of the larger star blots out the companion. It is therefore evident that the interpretation of photographs, as regards star-colouring and magnitude, should be done under a due sense of the difficulties I have alluded to. I give in the paper a record of the photographs I have taken from the pole down to declination 69°. They exceed 200 in number. How these negatives are to be used for astronomical purposes is a question constantly pressing on me, and the difficulties in answering that question can only be fully realized by those who have seriously considered it. I may enumerate some of them here. Most of the plates have small specks in the film, many of which look like stars, and are difficult to distinguish from stars, even when viewed through the microscope. Dr. Gill, at the Cape, meets this difficulty by taking two photographs successively of the same field, giving each an exposure of one hour, but this makes the work almost Herculean, especially where clouds generally cover the sky. Another plan is to let the telescope run a few seconds of arc in right ascension during the exposure of the plate, and this again is equivalent to two or more exposures of the stars on different parts of the film, and makes what would be called an hour's exposure only, equivalent to an exposure for a fraction of an hour. Admiral Mouchez suggests three exposures of one hour each, thus making a triangular figure with three positions of each star. But the objection has been taken to this, that it will cause confusion in close stars, causing the discs of some to overlap others, leaving the dirty specks still competing for recognition as stars. The long exposure of the plates adds to the difficulty in discriminating between specks and stars, by making the star disc appear black on the margins; while the short exposures make the star discs show faintly, and the atmospheric glare forms a halo round the star, which under the microscope materially assists in distinguishing between stars and specks. All stars below the 7th magnitude require a microscope to show them, but there is no difficulty in manipulating the light on the plate so as to distinguish with certainty stars from specks taken with the 20-inch reflector, with an exposure of 15 minutes. This statement refers only to viewing stars by the aid of the microscope, and does not apply to printing them direct from the negatives, which in some form must be done before star photography can be appreciated generally, and utilised as an astronomical pursuit.

It is a work of the first importance to secure good photographs of all stars down to the smallest magnitude in as short a time as possible; and they will be available in future for comparison with photographs which can at leisure be taken by large instruments for special researches.

The chart on which I am engaged will be twice the size of Arglander's, and with an exposure of 15 minutes for each plate, will contain a much larger number of stars than are given in the *Durchmusterung*. I submit a few negatives as samples of those already taken. Among them are plates of Polaris Andromeda, with the Nova, the Pleiades, and the Nebula in Orion. In regard to the discs of Polaris which I have placed on the blackboard, and to which I have referred already, the question of long exposures arises, which may obliterate close faint stars. It would be necessary to regulate the exposure in such a way as regards time, that a bright star would not extinguish a faint star very near to it. By comparing, a rough drawing of the photograph of the Pleiades with Mr. Hall's map, published in the *Monthly Notices*, vol. 40, there are perceptible differences in the size of the discs, and in the number of the stars shown. The photograph of the nebula in Orion shows nearly as much detail as Mr. Common's large photograph; but when carefully viewed the faintest of the stars shown on Mr. Common's photograph are not shown; but those down to within one or two magnitudes of the faintest are photographed on the plate.

Immediately I received the circular from Dun Echt, I turned my telescope on the nebula in Andromeda to photograph the new star. I found it a difficult matter, as the actinism of the nebula is not at all like that in Orion, but with 16 minutes' exposure, a trace of the centre of the nebula may be seen, and also the new star distinct from the nucleus. With exposures of 32 minutes, 60 minutes, and 90 minutes, the differences are still more clear.

Mr. Knobel: Perhaps it may not be inconvenient to take another paper of a similar nature before we take the discussion on Mr. Robert's Paper. We have received a letter from Messrs. Paul and Prosper Henry, of the Paris Observatory, on Stella Photography. They write from Paris on the 5th January: "We have recognised by means of photography the existence of a new nebula in the Pleiades. This nebula is in the neighbourhood of the star Maia, which it surrounds in part, and where it appears—in the photograph—to emerge from it in a spiral wisp, which comes out on one side, where it assumes a very characteristic spiral form, and extends to a distance of about two or three minutes of arc. It has been possible to obtain an image of the nebula on three different photographic plates, taken on the 16th November, and on the 8th and 9th of December. We add also, up to the present time, we have not been able to see the nebula in our telescopes. We have the honour also to send you an enlarged reproduction of the original negative, showing the nebula surrounding the star, and we are also permitted to send you some photographic pictures of Saturn obtained lately."

Mr. Knobel said: I desire to call particular attention to these photographs, not only the first mentioned of the nebula in the Pleiades surrounding the star Maia, which were obtained after three hours' exposure, but also to the very remarkable photographs of the planet Saturn, that have been obtained at Paris, showing the two rings—the inner and the outer ring—and showing the difference of the relative brightness of those rings, and also the crape ring undoubtedly, and Balls division, or what is known as the Cassini division; also the belts on the body of the planet, and the very dark polar cap of the planet. Some of the photographs are enlarged 11 times from the original negatives, and some of them 44 times, and they are certainly some of the most marvellous photographs that have yet been produced of celestial objects.

Mr. Ranyard said: I should like to express my very great admiration for these photographs by the Brothers Henry. The definition of the markings on the ball of the planet Saturn is something exquisite, and far beyond anything which has been obtained by anybody else. The dark narrow band in the equatorial region, which, though not very conspicuous at the last opposition, was seen by several observers, is well shown, as well as the dark polar cap. The latter is almost better than it can be seen with the eye and a large telescope under favourable conditions. It will be noticed that although the delicate differences of brightness on the ball of the planet are so well shown that other features that require sharp definition are not so striking, the great Cassini divisions in the rings is perhaps the most striking feature to the eye when the planet is observed in the telescope; but in the photographs it is only indicated by a dusky line, which is not so conspicuous as the dark equatorial belt upon the planet. The differences of brightness of the inner and outer ring are very strikingly shown, and appear more conspicuous than as seen with the eye. With regard to the beautiful photograph of the new little nebula in the Pleiades Mr. Wesley has been good enough to look up for me this evening one of the old photographs of the Pleiades taken by Mr. Rutherford, of New York, which is in the possession of the Society : but I have not been able to detect any trace of the nebula around the star Maia upon it. Mr. Rutherford's photograph was taken with the wet collodion process, and although small stars are shown, it does not by any means include all the small stars which are shown on the photograph of the Brothers Henry. No doubt the light of the nebula belongs to the violet actinic region of the spectrum, which does not make much impression on the retina.

Colonel Tupman : I should like to ask Mr. Roberts one question. Whether he has any special means of pointing his telescope during the exposures, in order to correct the erroneous driving of the clock ?

Mr. Roberts: When I am photographing in the neighbourhood of the Pole, which is the part I have now in hand, my driving-clock is sufficiently good, and there is no necessity to watch or correct it. Correction, no doubt, is an evil, and I endeavour to regulate the clock so that correction is unnecessary. Mr. Grubb has in hand for me a more perfect method of driving the instrument, for I shall require it when I come to work at some distance from the Pole.

Colonel Tupman : Then, I understand, you contemplate always depending on the clock ?

Mr. Roberts : When the exposure is longer than 15 minutes doubtless it would be necessary to watch—but with an exposure of 15 minutes only, I have great hopes that a clock will be obtained sufficiently accurate.

Mr. Lecky : There is a beautiful illustration of the photographic telescope of the brothers Henry in a publication called *La Nature*. It appeared about three weeks ago. I saw it the other day, but had not time to read the description. The observer is looking through an attached telescope, and he has a rod in his hand, evidently for adjusting the going of the clock.

Colonel Tupman : You mean for pointing during exposure.

Mr. Lecky : I should think so—for correcting any error in the clock.

Mr. Ranyard : It is so. The Messrs. Henry have a twin telescope mounted alongside the camera.

Mr. Knobel : I think we ought to be very glad indeed that Admiral Mouchez's letter has brought forth some fruit in this country in the labours of Mr. Roberts. (Hear, hear.) I looked upon the scheme projected by Admiral Mouchez with very great hope that it might catch the sight of some earnest amateur, who would devote himself to further the scheme proposed from Paris; and I think it is a matter for congratulation that Mr. Roberts has determined to devote himself, his skill, and his instruments, to furthering the scheme suggested by Admiral Mouchez.

Mr. Green: As the planet Saturn has been mentioned, I might refer to a most wonderful view I had of the planet on the evening of the 15th December last. The air was in a remarkably steady state, and it enabled me to look on the exterior ring with as much certainty of definition as though it were an engraving on paper, under a power of 500 or 600. I felt no doubt whatever at the time about the absence of any definite dark mark dividing the outer ring; but the mark which I had seen previously, which I called the shading, was there definitely. A brilliant light followed the division, which is usually called Cassini. Outside that the shaded band, and outwards from its edge a light portion towards the sky. The definition was beautiful, and I saw belts on the planet at about latitude 50°—more delicate objects than I had seen before, yet the steadiness of the atmosphere only confirmed the doubtful views I had of the outer ring on previous occasions.

Mr. Common: I should like to express my great admiration of these photographs of the Brothers Henry, of Saturn. I think they are the most marvellous things we have seen. They show the immense importance that photography will have in the future. I took photographs of Saturn in 1879—when the ring was a line across the planet—with an 18-inch telescope, and with a 36-inch; and for another purpose. I had occasion last week, strangely enough, to enlarge some of these little ones, and was wonderfully struck with the amount of detail that was visible even in them. I was thinking then, that just as in the early days when photography was used on the moon, and when people who used it got those blots of light where they now get exquisite details on the negatives, so possibly we shall compare the early photographs of Saturn with what will be soon taken. I believe that with experience and proper methods we shall be able to get more planetary detail on Saturn, Jupiter, and Mars than with the eye, for even with such a thing as a 3-foot reflector, and a very short exposure in the primary focus, which gives a very small image, one gets a certain amount of detail on the disc of Mars, taken in 1879. I think that if, to the telescope the Brothers Henry have, they will apply an enlarging lens to enlarge the primary image, and sacrifice the quickness of exposure, giving longer exposures to make up for the enlarged image, our position is this: you can get an image of Saturn in the principal focus almost momentarily, and you must enlarge the image to such an extent that the exposure would need to continue for a long time. We have proof from the photographs of stars that this is not impossible. They might certainly enlarge the image so that they would need an exposure of half-an-hour, and by so doing they might get such detail as it is impossible to show now. There may be difficulties in the way, but I think this is a very good omen for the future, and I hope Messrs. Henry will produce even more surprising things. No one can have been more surprised than the Brothers Henry when they saw this photograph of Saturn enlarged, and showing such detail, because the actual image of Saturn is not more than the thirtieth of an inch long, while the image of Jupiter in my telescope is less than a twenty-fifth. In these photographs there is no mistaking Jupiter. To my eye it is as plain as the large pictures of the planet on the walls. If we have only to get an improved definition, and enlarge the primary image, I believe you will get better results than any hand-drawing. In speaking of these photographs of the Brothers Henry, it is hardly necessary to say anything except that they are really wonderful. They have gone farther than anybody else to show that we are now making a new departure in astronomy. I won't say that the observer will be an obsolete animal, but perhaps his functions will be entirely altered. In the star maps you must follow the same line as with the nebulæ. Up to the present time we have not the power of putting on the plate large and small stars together, and for many reasons it is well we have not; but in taking photographs of the various parts of the heavens it will be necessary to have short exposures for the large stars, and longer exposures for the small stars, and to collate the two together as one picture. With regard to the very interesting paper by Mr. Roberts, there is a very great deal in it that I can thoroughly appreciate, because it agrees so well with what I have done myself and said myself; but there is an absence of particulars with regard to actual details of work that I rather regret; I should like to have heard a little more about the processes used. I do not quite agree with Mr. Roberts as to the difficulty that specks on the plates will cause. I believe it is possible to obtain plates which are, for all practical purposes, free from specks, and so get rid of what he considers a very great trouble. In all cases duplicate plates must be taken. That is better than taking two images of the stars on the same plate. I was delighted to see his photograph of the Pleiades; also of the Orion nebulæ. When I wrote an article for *Nature*, in November, 1884, I gave the result of some experiments with the telescope, using a second reflection from the mirror, nearly half the size of the large one, throwing the image back behind the mirror. With an exposure of half-an-hour, I got a very similar picture to that which Mr. Roberts exhibits, but smaller because the focal length of the mirrors happened to be 48 or 49 inches; but a lens of 48 or 49 inches gives a scale nearly exactly the same as Arglander's maps. The photograph of the Pleiades is very nice indeed. The distortion of the edge is very small, considering that the reflector was 20 inches aperture and 100 inches focal length. It says a great deal for Mr. Grubb's work. In regard to Mr. Roberts taking photographs without absolute control of the clock, I wish him success; but I am afraid he is trusting too much to the skill of the mechanic, because it is a thing that is almost impossible; and the only way to do it is to watch the image which is produced by the mirror as I did, or to do as the Brothers Henry do in Paris. From their description, and from what Mr. Lecky said, I gather that they have two telescopes fastened together, and there is no declination axis between the two that may give a slight twist. The two are intimately fastened together, and one cannot move without the other. The Messrs. Henry used a 13-inch object glass for their photographs, and a 10-inch object glass for their visual work, and by having a high power and delicate means of adjustment, they can keep the telescope going to a fraction of a second, and they have only to supplement the work of an excellent clock. I fancy Mr. Roberts will find he will have to devise some means of watching the exposure during the time it takes place, because to trust to the clock alone, with all the constant changes in the position of the main mirror—even if you work on the meridian or nearly so—the refraction itself, with long exposure, will come in in a most objectionable way; but I wish him every success in the work he has begun. (Hear, hear.)

Mr. Ranyard: With reference to Mr. Common's suggestion as to increasing the diameter of the planet before it is photographed, I have made a small step in that direction. I tried to take photographs of Saturn, and also of Jupiter, with an enlargement of five diameters, and they were not nearly so successful as those taken in the primary focus. With five diameters you require 25 times the exposure to produce the same density of image, and with 25 times the exposure several new difficulties are introduced. The errors due to the unsteadiness of the atmosphere are increased, and the difficulties of accurate driving are greatly increased. For, though any error in driving will be magnified equally, whether the image is enlarged before the photograph is taken, or after, the difficulty of driving at an even rate for 25 seconds is much greater than the difficulty of driving at an even rate for one second. And in addition to this there are the chromatic errors introduced by this enlarging lens.

Mr. Roberts, in reply, said: The criticisms have been of a very kindly character, and Mr. Common, who is a very great authority on the subject, has spoken in a manner that is most agreeable to me. As to the possibility of obtaining plates free from specks I did not find them after all, under the conditions in which I am working, any serious drawback. I think I may venture to say that I can distinguish between a star and a speck on any of my plates. Then I have two methods of checking the clock. One is a finder attached to the reflector, with a lens of 2½-inches in diameter, and in the eye-piece wires with a small distance between them, so I can move them in the field of view, and place a star between the wires, and keep it there if I think it necessary ; and I have also a 7-inch refractor at the other end of the declination axis, that can be used for the same purpose; but, as I am now photographing in the neighbourhood of the Pole, the clock drives the telescope with sufficient accuracy.

The President: I think you will all agree with me that we are very glad to have had the opportunity of hearing Mr. Roberts this evening. Mr. Roberts' name is not unknown to us. He resides at a long distance from London, and we most heartily welcome him here to-night, particularly when he brings us such beautiful photographs for our admiration. With regard to the photographs of Messrs. Henry, I quite agree with Mr. Common that they are most beautiful, and I think they show an advance which we could hardly have anticipated a year or two ago.

Mr. H. H. Turner read a paper entitled, *Notes on Mr. Marth's Intersects.* Mr. Marth has given in the *Monthly Notices* a diagram by which many of the elements of the planetary orbits can be recognized by simple inspection. The curves in the diagram are traced by the intersects of the planetary orbits, with a plane at right angles to the ecliptic, which revolves about an axis passing through the sun. The curves corresponding to the orbits of the planets moving in ellipses, are unsymmetrical curves of the fourth degree. By a simple change of co-ordinates Mr. Turner transforms these curves into conic sections, the advantage of which is that conic sections can be more easily calculated and laid down, and that the approximations of the orbit of one planet to another can be as easily recognized in the conic section curves as in the curves of higher degree more laboriously laid down by Mr. Marth.

Mr. Glaisher : Have you given in your paper the equations for Mr. Marth's curves, which you say are of the fourth order?

Mr. Turner said he had done so, and for the transformed curves, or conics, he had given expressions for the centre and the position and magnitude of the axes in terms of the elements of the orbit.

Mr. Glaisher: Mr. Marth's method is, of course, distinctly prettier ideally, because it comes closer to the actual physical facts, and has nothing empirical about it.

Mr. Turner: Exactly; but the amount of tabulation necessary for Mr. Marth's curves is very great; it occupies nearly a whole number of the *Monthly Notices.*

Mr. Knobel then read two papers, on a new variable star of short period in Orion—one by Mr. J. G. Gore, and the other by Dr. Copeland, who had observed a channeled spectrum, with some bright lines.

Capt. Noble: I observed this star last night. When I first got the Dun Echt circular, I looked at it, as no doubt many others did, and certainly it has since degraded in light. I had no means for accurate photometry. Mine was a mere eye estimation, but it certainly has degraded in brightness. I have always seen this star yellow. They say it is of an orange colour. I think there is some personal idiosyncrasy about the seeing of red stars. I hear of these wonderful red stars, but when I look at them, I find they are rather a full yellow. When Mr. Chambers was writing his "Descriptive Astronomy," we went over a great number that had been described as red, but we could see none of them but what we should describe as yellow. I thought last night that this star was not quite so full a yellow as when I first saw it.

Mr. Maunder: I observed the spectrum of this star on two occasions. I cannot add anything to what Dr. Copeland has said; indeed, I have not seen so much as he has done. I was simply able to recognise it as an exceedingly fine specimen of Padre Secchi's third type. The dark bands are exceedingly dark, completely dividing the spectrum, whilst the interspaces are remarkably bright. I do not know any star of the type in which the contrasts are so strong; but I was not able to notice any bright lines, or anything I could reasonably suspect as bright lines. I measured the position of the sharp dark edge of the principal band, and I got the position as 5,169 tenth metres. I do not know how that compares with Dr. Copeland. With regard to the colour of the star, I should certainly have described it as a reddish star. The light seemed to me to be considerably further down the spectrum than the D lines, for instance. It seemed to me a muddy red, and I should have described it as such.

Mr. Knobel: The Dun Echt observations give that very bright line, which Mr. Maunder found at 5,169, at 5,174. On December 18th, it was 5,156, and on December 26th 5,162 and later they give it as 5,165. The mean, therefore, comes to 5,162.

The following papers were taken as read:—

Major G. Strahan: *The Meteor Shower of* 1885, *November* 27.

Major A. C. Bigg-Wither: *The Meteor Shower of* 1885, *November* 27.

Prof. C. V. Zenger: *On a new spectroscopic method for measuring close Double Stars.*

J. E. Gore: *On a New Variable Star of Short Period.*

G. L. Tupman: *Observations of Comet* 1885 (*Brook's*) *at Harrow, with the* 18½*-inch Equatorial Reflector.*

Communicated by E. J. Stone: *Observations of the Meteors of* 1885, *November* 27, *made at the Radcliffe Observatory, Oxford.*

R. A. Proctor: *On the Biela Meteors.*

A. Marth: *Ephemeris of the Satellites of Mars,* 1886.

B. J. Hopkins: *On the Variation in Colour of the Nova Andromedæ, and on the evidence it affords as to the origin of the Nova.*

E. J. Stone: *Observations of the Moon, made at the Radcliffe Observatory, Oxford, during the year* 1885, *and a comparison of the results with the tabular places from Hansen's Lunar Tables.*

A Marth: *Note on the transit of the planet Mars and its Satellites across the Sun's disc, which will occur for the planet Jupiter and its Satellites on April* 13, 1886.

W. F. Denning: *Changes in the Red Spot on Jupiter.*

Communicated by the Astronomer-Royal: *Spectroscopic results for the motions of stars in the line of sight, obtained at the Royal Observatory, Greenwich, in the year* 1885, No. ix.

Communicated by the Astronomer-Royal: *Observations of Comets* d, 1885 (*Fabry*), *and* e, 1885 (*Barnard*), *made at the Royal Observatory, Greenwich.*

Communicated by the Astronomer-Royal: *Observations of occultations of stars and Uranus by the Moon, and of phenomena of Jupiter's satellites, made at the Royal Observatory, Greenwich, in the year* 1885.

E. W. Maunder: *Observations of the Spectrum of the Nova Andromeda, made at the Royal Observatory, Greenwich.*

THE SOLAR SURFACE IN 1885.

In looking over the Solar drawings, made at the Stoneyhurst Observatory in 1885, some remarks suggested themselves, which I thought might be of interest to the readers of *The Astronomical Register*, and usefully supplement the notice published in your number for November, 1884.

The record shows, that in addition to other changes, there has been a gradual increase in the number and magnitude of spots, during the first half of the year 1885, followed by a similar decrease in the second half. The largest spot of the year was visible in the month of June, and was near the central meridian on the 21st. There were several periods of considerable quietude, the two most remarkable being from January the 1st to the 17th, and from November the 23rd, to December the 10th; these, lasting about 17 days each, were apparently the turning points of two long waves of disturbance. The visible hemisphere of the sun was free from spots on seven days, and nearly so on several other occasions, all the days without spots occurring during the quiet periods.

The faculæ may be said, on the whole, to have increased, and to have diminished with the spots, the magnitude of each group corresponding, as a rule, with the character of the accompanying spots, but attaining its maximum later.

The results for 1885 show, on the whole, a decided decrease, as compared with 1884, both in the spots and faculæ, and also a marked synchronism in the variations of the spots and prominences.

The study of the faint evanescent spots, which is still continued, has revealed no new features in these objects, except that some of the "second class of faint spots" are notably darker than others, so much so, that they might easily be mistaken for small spots of the ordinary type, were it not for their peculiar characteristic of rapidly spreading, and fading away like a faint cloud. These darker spots are more rarely seen than their lighter companions; they are as common near the poles as elsewhere, and appear both singly and in groups. Several instances have been noticed near the middle of the disc, in which the centre of one of these darker spots (oblong in shape and fairly large) was apparently overlaid by a long narrow facula, without, however, entirely concealing the part of the spot over which it lay: thus the continuation of the spot was clearly perceptible underneath the facula, although it was fainter in the middle than at either side. The most careful study of similar cases, under very good atmospheric conditions, make it doubtful whether the facula was over the spot, or the spot over the facula. The form of well defined bright faculæ near the line C is frequently found to be temporarily changed, and the brightness to be dimmed in parts, and then to recover its original appearance, by the sudden formation and subsequent disappearance of these faint objects. From these observations, it might seem more probable that the spot lay over the facula, as a semi-transparent faint cloud. These evanescent spots have on the whole, been less striking, and more difficult to observe in 1885 than in previous years; but the solar surface has never been examined, with good definition, without detecting the usual rapid change in these objects, in a more or less marked degree.

In order to study the connection between spots and faculæ, to determine the number of cases of recurrence of the same spot, after disappearance, and to furnish data for comparison of individual solar outbursts with the variations of terrestrial magnetism, a systematic examination of the life history of each outburst of a spot is being undertaken at present. The number, heliographic position, approximate magnitude, and date of first and last appearance of every outburst is carefully noted, and a short description written of each.

So far as this study has yet gone, the distribution of the faculæ appears to be identical with that of the spots; but the former spread enormously, and remain visible in higher latitudes long after the spot has vanished. The bright isolated jets that are occasionally seen near the poles, are, as a rule, very evanescent, and appear to differ as much from the ordinary faculæ, that accompany the spots, as the faint evanescent spots do from those of the ordinary type.

The number of cases of recurrence of the same spot, after disappearance, is very striking, no less than 35 being recorded during the first seven months of 1884. All these are traceable to 20 different original disturbances of considerable magnitude. The repetitions of the same spot were of varying frequency, some reappearing three, four, and even five times. In some cases the recurrence took place soon after the disappearance, while other spots only reappeared after from 15 to 30 days; but the new outburst was always in the exact position of the previous spot, and in the midst of the faculæ connected with it. These faculæ often remain for at least two months after the spots in their immediate neighbourhood have vanished. This may perhaps explain why faculæ are sometimes thought to precede spot-outbursts. When the spots are carefully traced to their original formation, they are found never to be preceded by faculæ, The reappearances are generally smaller than the first disturbance, though the reverse of this has sometimes occurred.

Other details have been noticed in the course of the examination of the solar surface, and when sufficiently verified by further observations, they may form the subject of a later communication.

Stoneyhurst. S. J. PERRY.

SELENOGRAPHICAL NOTES, FEBRUARY, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Lassell. If this small ring-plain is examined when near the terminator under morning illumination, a very prominent ridge, coming up from the north-west, is seen to traverse the wide gap which exists in the north border and to penetrate some distance into the interior of the formation. On 13th January, at 7h. 45m., the E. longitude of the morning terminator being about 11°35′, this feature was especially striking, as the summit of the ridge stood up conspicuously above the shadow of the west wall, and the whole of the floor, with this exception, was in darkness. A very similar phenomenon may be noticed in connection with Mädler, but in this instance it is less obvious. South-east of Lassell a long dark straight line is visible at this stage of illumination, extending from the point of junction of two ridges, nearly due south of the ring-plain to a ridge in about E. long. 12° and traversing about the middle of its course a dusky oval-shaped ring. In Schmidt's map of the region nothing answering to this object is shown, but Mädler and Neison draw a rill-like valley, the latter describing it as "an extremely shallow valley." It seems to me, however, to represent the shadow of a fault or of a very steep ridge rather than that of the gently sloping side of a valley. As it is only slightly to the north of the so-called "straight wall" near Thebit, which undoubtedly is a sudden drop in the surface and not a true ridge, the two objects can be conveniently compared, and it will be found that the Lassell marking is almost as sharply defined as the straight wall, and is of a very similar character. There is something much resembling this object, though not so long, south of Aristárchus, which has also been recorded as a cleft, though it is clearly a fault. The position of the brilliant little crater on the north wall of Lassell, west of the gap, is indicated by an ill-defined bright spot under a low sun, its actual character only becoming apparent when the formation is more fully illuminated.

Alpetragius d. In the current number of *Sirius,* Dr. Klein reports the result of an observation of this object made on 15th November. The white elliptical spot clearly impressed him with being a convex surface, though no shadow was observed. The little crater on the south margin of it was estimated to be about one-half the diameter of those at the northern termination of the straight wall near Thebit. At times of best definition a number of little hills were visible in the neighbourhood.

Clefts, &c., within Hevel. Between 10h. and 11h. 30m. on the evening of 18th January the following objects were observed in the interior of this formation. A cleft (1) crossing the floor from a point south of the prominent crater on the south-east border to the west wall, passing in its course by a craterlet near the latter. A cleft (2) crossing the last was traced from a little hill near the south border to a craterlet a short distance north of the central mountain. This cleft is slightly curved, the convexity being turned towards the west. A cleft (3) running from the craterlet near the west border (mentioned in connection with cleft 2) to a point on the south-western side of the interior. A cleft (4) passing from this latter point to the place where cleft 1 joins the south-east wall. Of these four clefts, 1 and 2 (answering to ψ ψ^1 in Neison's map XI.) were easily seen, under the very favourable conditions which prevailed, with an eye-piece magnifying 340 times on my 8½-in. reflector. Cleft 3 was less obvious, but was satisfactorily observed during spells of best definition. Cleft 4 was also difficult. It seems to consist, in part at least, of a row of craterlets of the minutest type. Near the place where cleft 1 crosses cleft 4 there is a very conspicuous little crater. Schmidt draws clefts 1, 2, and 3, but not quite satisfactorily as regards position and direction. He moreover shows 1 and 2 as perfectly straight, whereas 2 is certainly curved throughout, and 1 in part. On the site of cleft 4 he shows nothing, a somewhat strange circumstance, as the locality of this cleft is indicated by two or three large craterlets, which could hardly escape a careful scrutiny of this quarter of the floor. There is a bright little crater on the triangular central mountain of Hevel, which I saw for the first time on the evening of 18th January, when it was easily detected. This object seems to have been hitherto unrecorded, though it is certainly not a difficult one on a favourable night. There are many craterlets and two short crater-rows within Hevel which do not appear in Schmidt's map. The plain south-west of Hevel is traversed by a cleft, ξ, originating seemingly at the border, which forms an apparent prolongation of cleft 1 in the interior. Under a high light it is conspicuous as a fine white streak.

Galileo's rank in the moon. If an attempt were to be made to do strict justice to the memory of bygone philosophers and men of science in Lunar nomenclature, the present anomalous system would need entire re-construction, and for some years to come we should have "confusion worse confounded." I agree with Mr. Allen, in the *Register* for January, that Galileo has received very scant justice from the old selenographers in having so small an object identified with his immortal name, but I fail to see that matters would be improved by shifting it from a small but conspicuous little ring-plain, to a region which would only rank as one of the smallest "seas" on the moon's visible surface.

Kempston, Beds: 20 January, 1886.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

PROPOSED MAPS FOR TRACING METEOR PATHS.

I have long thought that the existing star-charts are inadequate for meteor observers.

The British Association maps prepared for them, though very useful, seem not sufficiently accurate for present requirements. Many observers make their own maps by inserting in skeleton charts, drawn on a preferred projection, just such stars as they need. This appears an unnecessary individual expenditure of labour, as, if suitable maps were prepared, they would do for all engaged in shooting-star investigations.

I therefore propose to get some printed, and shall be glad to receive suggestions from meteor observers who have any to make on the subject.

The following points sketch my present plan; about those to which a ? is added I feel more doubtful than as to the others:—

There should be 14 maps, engraved with the utmost attainable accuracy, on the gnomonic projection, with centres at

R.A.	0°	D. ± 90°	R.A.	0°	D. 0°
	45	± 45		90	
	135			180	
	225			270	
	315				

Diameter, 140°. Square; length of edge, 24 (?) inches. The maps will thus overlap each other a great deal, one advantage of which would be the facility of transference of a meteor-path from one map to another.

R.A. and D. circles to be finely drawn (in same coloured ink and at same printing as the stars) for every degree of D., and for every degree of R.A. except near the poles. The R.A. and D. to be given in degrees marked at each interval of five, and the hours of R.A. to be marked also (?).

The sky to be represented as seen with the naked eye, but without the Milky Way. Stars and cumuli to be inserted to 6·5 (?) magnitude, and fainter stars separately; also when they are cumulatively as bright, like as in the *Uranometria Argentina* maps. To attempt completeness in this respect, all the stars to the adopted limit of magnitude in the catalogues of the *Uranometria Argentina*, of the *Harvard Photometry*, and of *Heis*, will be taken, and other catalogues consulted for supplying omissions, with the chief object of ascertaining the individual stars of which the double and multiple stars in *Heis* are composed, as he does not give them: about such catalogues information would be very useful, and is solicited.

The positions of the stars to be taken from the various Greenwich (especially the Nine-Year for 1872), Radcliffe, and Cape Catalogues for the epoch 1900 (?). An accompanying catalogue of the stars mapped to be printed, like those of *Argelander* and of *Heis*. (?)

The name to appear in each constellation, and be frequently repeated, abbreviated (or by symbol for the zodiacal constellations) (?), near its borders, to facilitate recognition of outlying stars. The usual constellations to be adopted, not Bode's.

Every star to have a designation, and one only, to be taken in order, thus: 1, Greek letters; 2, in some cases Roman capital letters, as in Camelopardus and for variable stars; 3, Flamsteed's Nos.; 4, Roman small letters; 5, B.A.C. Nos. (?); 6, Nos. from the *Uranometria Argentina*, to be distinguished by G. (for Gould) (?); 7, Nos. from *Harvard Photometry* (H.P.) (?); 8, Lalande's (Ll.) Nos. (?); 9, ... (?).

Many observers give the position or direction of meteors observed in degrees of R.A. and D., while others make reference to stars. The latter method seems to be simpler and more exact, but it is unsatisfactory on turning to a map to find that stars conspicuous in the heavens are wholly omitted, or marked in without a name of any kind. The above suggestions are made with a view to supply such deficiencies.

Magnitudes to be represented by black circles of various sizes, perhaps with a white centre to the brighter stars (?), shaped differently, as in Proctor's *New Star Atlas,* care being taken that the white and black have one centre. Stars that vary considerably to be depicted of the minimum magnitude, with surrounding circle of maximum.

All designations of stars to be in ink of some other colour than that for the stars themselves—say blue (?).

Precession to be shown in various parts of the maps, in some such way as in Proctor, exhibiting apparent motion of stars in 100 years relatively to lines of R.A. and D. Perhaps this would involve giving lines of latitude and longitude also.

The position of the horizon—say of Greenwich—at midnight, for fortnightly intervals, to be shown by a line on both edges of each northern and equatorial map, with an accompanying list of the hours at which the horizon has the same position on other dates.

A copy of each of the three patterns of lines of R.A. and D. used in the 14 maps to be printed on tracing-paper, and supplied with each set of maps. Means should be afforded of measuring 1° in every direction at any part of a map.

The sets of maps not to be bound, but each map be separately removable from a cover.

T. W. BACKHOUSE.

Sunderland: 19th January, 1886.

OCCULTATION OF ALDEBARAN.

Sir,—In the April number of your paper last year, there was a notice of the occultation of Aldebaran, by Prof. Piazzi Smythe and others. Perhaps it may interest some of your readers to state that the occultation was seen here, through the breaks of cloud, on the evening of January 16th. The disappearance was not seen remarkably well, but the re-appearance was quite sudden and neither phenomenon was accompanied by anything peculiar.

The instruments employed were the 10-inch reflector and the 3-inch refractor; the latter being used by a friend.

"Ivy House," EDMUND J. SPITTA.
Clapham Common, Jan. 21, 1886.

DUN ECHT CIRCULAR, No. 105.

Professor Krueger transmits from Kiel particulars of the discovery of yet another comet by Mr. W. R. Brooks. Its approximate place at 8h. 11m.·6 Cambridge (U. S.) mean time on December 27, was :—

R. A. 19h. 55m. 40s. ; Decl. N. 4° 8′.
Daily motion in R. A. + 11m. 0s., in Decl. + 2° 0′.

Lord Crawford's Observatory,
Dun Echt : 1885, December 30.

RALPH COPELAND.

DUN ECHT CIRCULAR, No. 106.

Elements and Ephemeris of Comet " Fabry."

The elements are derived from observations at Paris on December 1, at Berlin, Bilk, and Hamburg on December 7, and at Berlin on the 12th and 26th.

Elements.

$$\begin{aligned} T &= 1886,\ \text{April } 4{\cdot}180 \text{ Berlin M. T.} \\ \pi - \Omega &= 127^\circ\ 29'\ 10'' \\ \Omega &= 36\ \ 12\ \ 1 \\ i &= 80\ \ 52\ \ 49 \\ \log q &= 9{\cdot}79386 \end{aligned} \left.\vphantom{\begin{aligned} a\\ b\\ c\end{aligned}}\right\} \text{Mean Equinox } 1886{\cdot}0.$$

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log. Δ	log. r
Jan. 5	23	40	2	N. 21	1·3	0·2220	0·2486
6	23	39	6	21	4·4		
7	23	38	13	21	7·7		
8	23	37	22	21	11·2		
9	23	36	33	21	15·0	0·2255	0·2339
10	23	35	46	21	19·0		
11	23	35	0	21	23·2		
12	23	34	17	21	27·6		
13	23	33	35	21	32·3	0·2283	0·2185
14	23	32	55	21	37·2		
15	23	32	16	21	42·4		
16	23	31	39	21	47·8		
17	23	31	4	21	53·4	0·2304	0·2025
18	23	30	30	21	59·2		
19	23	29	58	22	5·3		
20	23	29	27	22	11·6		
21	23	28	58	22	18·2	0·2316	0·1857
22	23	28	30	22	25·0		
23	23	28	3	22	32·1		
24	23	27	38	22	39·4		
25	23	27	14	N. 22	46·9	0·2319	0·1682

The comet's brightness is slowly increasing.

Dr. H. OPPENHEIM.

Berlin, 1885, December 31.

Lord Crawford's Observatory,
Dun Echt: 1886, January 4.

DUN ECHT CIRCULAR, No. 107.

The following elements of comet "Barnard," are based on places obtained at Dresden, Hamburg, and Berlin, on December 5, 15, and 26, respectively.

Elements.

T = 1886, May 3·205 Berlin M. T.
$\pi - ☊$ = 119° 41′ 15″ } Mean Equinox 1886·0
☊ = 68 20 20 }
i = 84 20 24 }
log q = 9·67934

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log Δ.	log. r
Jan. 5	3	1	25	N. 9	1·2	0·2061	0·3543
6	2	59	14	9	11·7		
7	2	57	6	9	22·3		
8	2	55	0	9	33·1		
9	2	52	56	9	43·9	0·2078	0·3430
10	2	50	53	9	54·9		
11	2	48	53	10	6·0		
12	2	46	56	10	17·2		
13	2	45	1	10	28·5	0·2104	0·3314
14	2	43	8	10	39·9		
15	2	41	18	10	51·4		
16	2	39	30	11	3·0		
17	2	37	45	11	14·7	0·2136	0·3193
18	2	36	3	11	26·5		
19	2	34	22	11	38·4		
20	2	32	44	11	50·4		
21	2	31	8	12	2·5	0·2173	0·3068
22	2	29	35	12	14·7		
23	2	28	4	12	27·0		
24	2	26	36	12	39·4		
25	2	25	9	12	51·8	0·2213	0·2937

The comet's brightness is slowly increasing.

Dr. H. OPPENHEIM.

Berlin, 1885, December 31.

Brooks' comet was seen here last night in R. A. 20h. 22m. 1s. and N. Decl. 7° 12′·6 at 7h. 5m.·8 Dun Echt M. T.

Lord Crawford's Observatory,
Dun Echt: 1886, January 4.

DUN ECHT CIRCULAR, No. 108.

The "Science Observer," Boston, U. S., sends a code telegram of 18 words containing the following particulars of comet "Brooks."

Elements.

$T =$ 1885, November 29·12 Greenwich M.T.

$$\left.\begin{aligned} \pi - \Omega &= 39^\circ\ 58' \\ \Omega &= 262\ \ 42 \\ i &= 42\ \ 35 \end{aligned}\right\}\ \text{Mean Equinox 1885·0}$$

$\log q =$ 0·04210

Ephemeris for Greenwich Midnight.

1886	R. A. h.	m.	s.	Decl. °	′	Brightness.
January 4	20	26	44	N. 7	47	0·87
8	20	42	28	9	35	
12	20	58	8	11	21	
16	21	13	36	N. 13	3	0·70

The elements are derived by Mr. S. C. Chandler, jun., from observations on December 28, and 30, and January 1.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, January 4.

DUN ECHT CIRCULAR, No. 109.

Elements and Ephemeris of Comet "Brooks."

The elements are computed from observations at Cambridge (U. S.) on December 28, at Vienna on January 1, and at Berlin on the 7th.

Elements.

$T =$ 1885, November 24·7806 Berlin M. T.

$$\left.\begin{aligned} \pi - \Omega &= 34^\circ\ 36'\ 57'' \\ \Omega &= 262\ \ \ 1\ \ 48 \\ i &= 42\ \ 25\ \ 11 \end{aligned}\right\}\ \text{Mean Equinox 1886·0}$$

$\log q =$ 0·03012

Middle place: $\Delta\lambda . \cos\beta = -4''$; $\Delta\beta = -2''$.

Ephemeris for Berlin Midnight.

1886	R. A. h.	m.	s.	Decl. °	′	log Δ	log r	I
Jan. 10	20	49	51	N 10	24·0	0·2891	0·1188	0·78
12	20	57	32	11	15·8			
14	21	5	10	12	6·8	0·2959	0·1310	0·72
16	21	12	45	12	56·8			
18	21	20	17	13	45·9	0·3033	0·1433	0·65
20	21	27	46	14	34·0			
22	21	35	11	15	21·0	0·3115	0·1557	0·59
24	21	42	32	16	7·0			
26	21	49	50	16	51·9	0·3203	0·1680	0·54
28	21	57	4	17	35·7			
30	22	4	14	18	18·5	0·3295	0·1803	0·49
Feb. 1	22	11	20	19	0·1			
3	22	18	21	N. 19	40·7	0·3392	0·1925	0·44

The brightness "I" on December 28 has been taken as unity.

Dr. H. OPPENHEIM.

Berlin, 1886, January 9.

Lord Crawford's Observatory,
Dun Echt: 1886, January 15.

DUN ECHT CIRCULAR, No. 110.

Ephemeris of Comet "Fabry" continued from *Dun Echt Circular No.* 106.

Ephemeris for Berlin Midnight.

1886.	R. A.				Decl.		log. Δ	log. *r*	*I*
	h.	m.	s.		°	′			
Feb. 3	23	24	22	N.	24	5·2	0·2287	0·1255	2·4
5	23	23	54		24	25·3			
7	23	23	28		24	46·4	0·2252	0·1050	2·7
9	23	23	5		25	8·5			
11	23	22	43		25	31·6	0·2204	0·0834	3·0
13	23	22	24		25	55·7			
15	23	22	6		26	20·7	0·2140	0·0607	3·5
17	23	21	49		26	46·7			
19	23	21	32		27	13·7	0·2058	0·0370	4·1
21	23	21	16		27	41·7			
23	23	21	0		28	10·6	0·1958	0·0121	4·8
25	23	20	44		28	40·4			
27	23	20	27		29	11·0	0·1836	9·9860	5·7
Mar. 1	23	20	8		29	42·5			
3	23	19	49	N.	30	14·8	0·1689	9·9591	6·9

The brightness "*I*" on December 1 has been taken as unity.

Ephemeris of Comet "Brooks" continued from *Dun Echt Circular, No.* 109.

Ephemeris for Berlin Midnight.

1886.	R. A.				Decl.		log. Δ	log. *r*	*I*
	h.	m.	s.		°	′			
Feb. 3	22	18	21	N.	19	40·7	0·3392	0·1925	0·4
5	22	25	18		20	20·2			
7	22	32	11		20	58·6	0·3493	0·2046	0·4
9	22	38	59		21	35·8			
11	22	45	43		22	12·0	0·3595	0·2165	0·4
13	22	52	22		22	47·1			
15	22	58	57		23	21·2	0·3700	0·2282	0·3
17	23	5	27		23	54·3			
19	23	11	52		24	26·4	0·3806	0·2398	0·3
21	23	18	12		24	57·5			
23	23	24	27		25	27·6	0·3912	0·2511	0·3
25	23	30	39		25	56·8			
27	23	36	46		26	25·1	0·4019	0·2622	0·2
Mar. 1	23	42	48		26	52·5			
3	23	48	46	N.	27	19·1	0·4125	0·2731	0·2

The brightness "*I*" on December 28 has been taken as unity.

Dr. H. OPPENHEIM.

Berlin, 1886, January 16.

Lord Crawford's Observatory,
Dun Echt, 1886, January 25.

REVIEW.

Annual Report of the Board of Regents of the Smithsonian Institution. Showing the operations, expenditures, and condition of the Institution for the year 1883. Washington: Government Printing Office. 1885. pp. xxxvii. 959.

This large volume embodies a great amount of valuable and interesting information in the various departments of science, evidencing the vigorous life and excellent management of this admirable Institution. It contains various reports and proceedings, including the ceremonies at the unveiling of the Henry statue, and the report of the Secretary concerning the U. S. National Museum. The record of scientific progress for 1883 occupies over half the volume. Commencing with astronomy, by Professor Holden; geology is treated by T. Sterry Hunt, LL.D., F.R.S.; geography by Commander F. M. Green, U.S.N.; meteorology, by Cleveland Abbé; then follow physics, chemistry, mineralogy, botany, zoology, by various able writers, and finally anthropology occupies a large space, with numerous illustrations of mounds, earthworks, relics, &c., of Indian tribes.

THE PLANETS FOR FEBRUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	20 3 0	S. 21 57½	5″·0	23 12·7
	9th	20 56 37	S. 19 21	5″·0	23 34·7
	17th	21 51 18	S. 15 13½	4″·8	23 57·8
	25th	22 39 51	S. 10 23	5″·0	0 18·7
Venus ...	1st	22 29 44	S. 3 20	52″·3	1 43·0
	9th	22 18 3	S. 2 39	58″·2	0 59·9
	17th	21 57 28	S. 3 20	60″·6	0 10·3
	25th	21 40 5	S. 4 53	58″·3	23 15·2
Mars ...	1st	11 47 18	N. 5 29½	13″·9	12 42·1
	9th	11 43 18	N. 6 7	14″·9	11 58·9
	17th	11 36 16	N. 7 1	15″·7	11 15·9
	25th	11 26 38	N. 8 6	16″·2	10 34·5
Jupiter ...	1st	12 23 24	S. 0 56½	38″·5	15 34·4
	9th	12 21 54	S. 0 45	39″·3	15 1·5
	17th	12 19 44	S. 0 29	39″·9	14 27·8
	25th	12 16 59	S. 0 9	20″·5	13 53·6
Saturn ...	1st	6 9 38	N. 22 40	18″·0	9 21·7
	9th	6 8 4	N. 22 41½	19″·8	8 48·6
	17th	6 6 58	N. 22 43	17″·6	8 16·0
	25th	6 6 22	N. 22 44½	17″·4	7 44·0
Uranus ...	10th	12 28 1	S. 2 13	3″·8	15 3·6
	22nd	12 26 41	S. 2 4	4″·0	14 15·1
Neptune ...	2nd	3 23 20	N. 16 47½		6 31·8
	18th	3 23 39	N. 16 50		5 29·3

Mercury is unfavourably situated for observation, rising about three-quarters of an hour before the sun, the interval decreasing. Towards the end of the month he sets soon after the sun.

Venus sets two hours and three-quarters after the sun at the beginning of the month, the interval decreasing.

ASTRONOMICAL OCCURRENCES FOR FEBRUARY, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	1		Sidereal Time at Mean Noon 20h. 46m. 27·95s.			Pollux. 10 50·1
Tues	2	8	Conjunction of Moon and Mercury 5° 24′ S. Sun's Meridian Passage 13m. 58·08s. after Mean Noon	3rd Ec. R. 3rd Oc. D. 3rd Oc. R. 2nd Sh. I.	13 14 21 14 19 17 1 18 43	10 46·1
Wed	3	15 15	● New Moon			10 42·2
Thur	4			2nd Ec. D. 1st Ec. D. 2nd Oc. R. 1st Oc. R.	13 7 39 17 32 9 17 47 20 41	10 38·3
Fri	5	7	Conjunction of Moon and Venus 5° 59′ N.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	14 40 15 36 16 57 17 51	10 34·3
Sat	6			2nd Sh. E. 1st Ec. D. 2nd Tr. E. 1st Oc. R.	10 49 12 0 28 12 38 15 8	10 30·4
Sun	7			1st Sh. E. 1st Tr. E.	11 25 12 18	10 26·5
Mon	8			1st Oc. R.	9 34	10 22·5
Tues	9			3rd Ec. D. 3rd Ec. R. 3rd Oc. D.	14 17 10 17 11 8 17 50	10 18·6
Wed	10	6 6 7 12	Occultation of B.A.C. 741 (6½) Reappearance of ditto			10 14·7
Thur	11	14 46 1	☽ Moon's First Quarter Neptune at quadrature with the Sun	2nd Ec. D.	15 43 24	10 10·7
Fri	12	8 40 9 40 10 45 11 47 13 43 14 25	Occultation of 48 Tauri (6) Reappearance of ditto Occultation of γ Tauri (4) Reappearance of ditto Occultation of 71 Tauri (6) Reappearance of ditto Saturn's Ring: Major axis=44″·74 Minor axis=20″·00	1st Sh. I. 1st Tr. I. 1st Sh. E.	16 34 17 23 18 51	10 6·8
Sat	13	13 51 14 34 15 10 15 59	Occultation of 111 Tauri (5½) Reappearance of ditto Occultation of 117 Tauri (6) Reappearance of ditto	3rd Tr. E. 2nd Sh. I. 2nd Tr. I. 2nd Sh. E. 1st Ec. D. 2nd Tr. E. 1st Oc. R.	10 3 10 33 12 12 13 22 13 53 36 14 56 16 54	10 2·9
Sun	14	9	Conjunction of Moon and Saturn 4° 21′ N. Illuminated portion of disc of Venus=0·007 Illuminated portion of disc of Mars=0·982	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	11 2 11 49 13 19 14 4	9 58·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	15		Sidereal Time at Mean Noon 21h. 41m. 39·70s.	2nd Oc. R. 1st Oc. R.	9 19 11 20	Pollux. 9 55·1
Tues	16		Sun's Meridian Passage 14m. 17·05s. after Mean Noon	3rd Ec. D.	18 15 25	9 51·1
Wed	17	14 19 15 21	Occultation of ι Leonis (6) Reappearance of ditto			9 47·1
Thur	18	6 15 15 44 16 43 7 16	○ Full Moon Occultation of 48 Leonis (6) Reappearance of ditto Inferior conjunction of Venus and the Sun Conjunction of Venus and Mercury 11° 22′ S.	2nd Ec. D.	18 19 20	9 43·2
Fri	19	13 15 14 0 18	Occultation of τ Leonis (3) Reappearance of ditto Conjunction of Moon and Mars 3° 50′ N.	1st Sh. I.	18 28	9 39·3
Sat	20	9 36 10 23 17 53 18 34 14	Occultation of 13 Virginis (6) Reappearance of ditto Occultation of URANUS Reappearance of ditto Conjunction of Moon and Jupiter 0° 8′ S.	3rd Tr. I. 3rd Sh. E. 2nd Sh. I. 3rd Tr. E. 2nd Tr. I. 1st Ec. D. 2nd Sh. E. 2nd Tr. E.	10 48 11 1 13 6 13 28 14 29 15 46 49 15 55 17 13	9 35·3
Sun	21			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	12 56 13 35 15 13 15 50	9 31·4
Mon	22	13 3 13 20	Occultation of κ Virginis (4½) Reappearance of ditto	1st Ec. D. 2nd Oc. D. 1st Oc. R. 4th Ec. D.	10 15 8 11 38 13 5 16 40 46	9 27·5
Tues	23			1st Sh. E. 1st Tr. E.	9 41 10 16	9 23·5
Wed	24	14 13	Near approach of 49 Libræ (5½) Superior conjunction of Mercury and Sun			9 19·6
Thur	25	5 11	☾ Moon's Last Quarter			9 15·7
Fri	26					9 11·7
Sat	27			3rd Sh. I. 3rd Tr. I. 3rd Sh. E. 2nd Sh. I. 2nd Tr. I. 3rd Tr. E. 1st Ec. D.	11 57 14 10 14 59 15 40 16 44 16 50 17 40 7	9 7·8
Sun	28			1st Sh. I. 1st Tr. I. 1st Sh. I. 1st Tr. E.	14 50 15 20 17 7 17 35	9 3·9
MAR. Mon	1			2nd Ec. D. 1st Ec. D. 2nd Oc. R. 1st Oc. R.	10 14 7 12 8 28 13 56 14 49	8 59·0

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Feb. 1	−64	37 E.	Feb. 11	− 6	15 M.	Feb. 21	+52	2 E.
2	76	47	12	18	26	22	39	53
3	88	57	13	30	35	23	27	42
4	+78	54 M.	14	42	46	24	15	32
5	66	43	15	54	56	25	3	22
6	54	34	16	67	6	26	− 8	49
7	42	24	17	79	15	27	21	0
8	30	15	18	+88	34 E.	28	33	10
9	18	4	19	76	24			
10	5	54	20	64	13			

Moon farthest from the Earth, Feb. 2, 23h.
,, nearest the ,, ,, 17, 14h.

Books received:—The Apparent Position of the Zodiacal Light. By Arthur Searle.—Astronomische Nachrichten.—L'Astronomie. 5e année. No. 1.—Sidereal Messenger.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1885.

Compton, A. J.
Dale, R.
Dobie, Dr.
Green, N. E.
Heelis, J.
Monk, Dr.
Perrins, J. D.
Perry, Rev. S. J.
Smith, Dr. R.

To Jan., 1886.

Lean, W. S.

To June, 1886.

Banks, Mrs.
Grove, S.

To Oct., 1886.

Ballot, J.

To Dec., 1886.

Barber, J. T.
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Collingwood, E. J.
Davies, Rev. R. P.
Draper, Mrs.
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All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The Astronomical Register.

No. 279. MARCH. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. G. L. Tupman.

The Annual General Meeting was held at the Society's apartments, Burlington House, on Friday, February 12th.

Rev. Charles Herbert Griffith, M.A., 1, College Grounds, Brighton.
John Hartnup, Liverpool Observatory, Bidston, Birkenhead.
Duncan Milligan, 21, Spencer Road, Wandsworth Common.
Ole Möller, 8, Austin Friars, E.C.
Capt. W. G. Morris, R.E.
Rev. Charles J. Taylor, M.A., Topplesfield Rectory, Essex.
Christopher Thwaites, Mem. Inst. C. E., Beccles, Suffolk,

were balloted for, and duly elected Fellows of the Society.

Colonel Tupman read the minutes of the last annual meeting, and those of the special meeting of May 8th, 1885, which were confirmed.

Mr. Knobel: As we have business to day which will occupy a great deal of time, and as it is incumbent on us to arrange in the most convenient manner for Fellows who wish to hear the result of the ballot for the officers and council, I have to suggest that we commence with the ballot, which can close at any time at the pleasure of the meeting. If the ballot be taken now, probably, as the scrutineers will be some time engaged in the consideration of the ballot papers which have been sent in, the Fellows will not be detained too long, before knowing the result. If it is the pleasure of the meeting that the ballot be taken now, we could, after the voting, at once proceed to the reading of the Report, and the President's address, and take the special subject on the paper.

Lord Crawford seconded the proposal.

Captain Noble: I think provision should be made for the ballot remaining open long enough, so that Fellows coming in later, during our proceedings, should have the opportunity of voting. Perhaps some of the Fellows do not care to come in time to hear the list of Fellows who have died during the year, and may intentionally defer their visit till the necrology is over.

Mr. Knobel: It will be according to the pleasure of the meeting when the ballot shall close. But before it commences I would point out, that, in accordance with the alteration of the bye-law made last year, the signatures of Fellows on the envelopes have to be verified; and I would therefore suggest that the Assistant Secretary do attend the scrutineers for the purpose of verifying those signatures before the scrutiny commences.

Lord Crawford: Is not that putting too severe a task on the Assistant Secretary? Is it not assuming forgery on the part of some of the Fellows?

Mr. Ranyard: I think there can be no harm in verifying the signatures outside the envelopes.

The President: I should not think the verification would take long.

Captain Noble: We should ascertain whether the name is on the list of our Fellows; otherwise we may have Dr. Collet, of Leicester, voting. (Laughter.)

Mr. Ranyard: I will second that suggestion.

Mr. Knobel: I do not think there can be the slightest imputation on the Fellows in doing that.

Lord Crawford: Perhaps it was too strong a statement of mine.

Mr. Knobel's suggestions were then put to the meeting and adopted.

The President nominated Sir James Cockle, Mr Lecky, and Mr. Inwards as scrutineers, and this was agreed to.

Mr. Lecky: There is one question I would like to ask. Suppose the ballot paper is signed by the Fellow; is that to be passed by the scrutineers?

Mr. Ranyard: If it is a signature on a printed list I should think there could be no objection to counting the paper; but I should prefer that all such points should be left to the judgment of the scrutineers. It would be a great pity to refuse votes on a mere technical ground.

The President: It appears to me the bye-laws state decidedly what is to be done, and the scrutineers, I think, should have power to interpret them; and they must use their own judgment in considering what the meaning of the bye-law is.

Mr. Lecky: I have no wish, either one way or the other; but I have been asked whether a balloting list signed by the voter is a proper balloting list, or whether it does not destroy the spirit of the ballot.

Mr. Knobel: When the alteration in the bye-law was made it was decided, I believe, that it would be a contradiction of the spirit of the ballot; and any signature on the ballot paper was considered unadvisable.

Lord Crawford asked whether the signature to the ballot paper would not vitiate the paper, inasmuch as the names on the paper would then be redundant unless some other name were struck out.

Mr. De la Rue: Supposing no name were struck out, the signature would be a redundant name.

Mr. Ranyard: It would be a pity to reject such papers; I think it may safely be left to the judgment of the scrutineers to say whether the name is added as a signature or not. There is no privacy at present which will be violated by such signatures, for Fellows voting are directed to sign their names on the outside of the envelope.

The President: It is a question for the scrutineers to decide.

Mr. Ranyard: But the meeting can direct the scrutineers what to do.

Mr. Common: According to what took place at the special meeting, I understood that the scrutineers should open the envelopes and deposit the ballot papers in the boxes; and in that case they would be mixed up and treated altogether. It appears to me to be the best plan to bring out the envelopes and let them be verified, and opened and dropped into the box by the Assistant Secretary as if the Fellow were present.

Mr. Knobel: There are doubtful points, I know, that the scrutineers will have to consider, and if that course were adopted it would occupy a great deal of our time here.

Colonel Tupman: May the sheet be torn in half?

Captain Noble: I think the bye-laws say "any list."

[The ballot was then taken in the room, after which the scrutineers retired, and it was agreed that the ballot should remain open till half-past four o'clock.]

Mr. Creswick presented the Report of the Auditors, with a certificate stating that the books and instruments were in a satisfactory condition, so far as safe keeping was concerned. A 6-inch Grubb Telescope had been sent to replace the telescope lost by the Transit of Venus Expedition, in the *City of Boston* steamship.

Captain Noble: I should like to ask a question. There is mention made in the Auditors' Report of an instrument belonging to this Society which was lent to the Transit of Venus Committee, and lost. May I ask whether the Committee is a Government body, or a private association, and to whom are the Fellows to look to be recouped for the value of the instrument? What is this Transit of Venus Committee? I see by the report another instrument has been lent. I thought the Transit of Venus was over and gone. I did not know this Committee was sitting *en permanence.* Who is to recoup the Society for the value of the instrument lost?

Mr. Knobel: I think Captain Noble has misunderstood the report. The telescope lent to the Transit of Venus Committee was lost, but the Transit of Venus Committee have kindly replaced it by a more valuable instrument. (Laughter.)

Captain Noble: I am sorry I misunderstood it. I am very glad to hear it. I should be willing to lend them another if that is the case. (Laughter.)

Mr. Knobel then read the first part of the Report of the Council to the Sixty-sixth Annual General Meeting.

The income of the Society derived from the interest on investments is nearly £400 per annum. The income derived from annual subscriptions, admission fees, and first contributions, is a little over £800 per annum. Besides this the Society has received £65 during the past year derived from the sale of publications, and £168 from composition fees; while the expenditure has amounted to about £1,400 during the year.

A Catalogue of the Library has been prepared by the Assistant Secretary, which is now in the press, and will shortly be in the hands of Fellows.

The Society has lost by death one Associate during the year, and twenty Fellows. Amongst the deceased Fellows may be mentioned Admiral Bayfield, General Hannington, Mr. Ladd, the Earl of Shaftesbury, Mr. Wray (the well-known maker of object-glasses), and the Rev. T. W. Webb (author of "Celestial Objects for Common Telescopes.") Mr. Knobel mentioned that the Council were indebted to Mr. Ranyard for their memoir of the last-named gentlemen. Nine minor planets have been discovered during the year: four by M. Palisa, of Vienna. This brings the total number now discovered up to 253. Six comets have been discovered during the year 1885, including the periodic comet of Tuttle. Two of them were discovered by Mr. Barnard, of Nashville, and two by Mr. Brooks, of Phelps, New York.

Dr. Galle has published in the *Astronomische Nachrichten* a catalogue of the orbits of recently-observed comets, as well as of those which have been improved by the more careful discussion of ancient observations. This is likely to prove a valuable contribution to cometary literature. The catalogue is really a continuation of that given in the third edition of Olbers' *Methode zur Berechnung der Cometenbahnen* (1864), apparently prepared, or at least now published, at the express solicitation of the editor of the *Ast. Nach.* It commences, however, in 1860, and in an approximate form gives the most accurate orbit of each comet that has appeared within the quarter of a century ending in 1884, together with the period of visibility and the length of time over which the observations employed in deriving the elements extend. From this list a computer can easily see what orbits have been definitely determined, and of what comets further discussion is likely to be useful. In the accompanying notes are short descriptions of, and references to, the original authorities where comets have been observed, but not a sufficient number of times to enable the elements to be determined.

The number of comets for which orbits have actually been computed up to the end of 1884 is 302; of these, twelve have proved themselves periodic, and have actually been re-observed more or less frequently at subsequent returns. Taking these into account, the number of computed orbits occasioned by cometary discoveries is 357. In the quarter of a century under immediate consideration 95 have been computed, while the number of improved orbits is 42.

After the reading of the report, the President proceeded to deliver his address, on presenting the Society's gold medals, which have been awarded by the Council to Professor E. C. Pickering and Professor Charles Pritchard for their photometric researches. He said: You may remember that in June, 1871, a new bye-law was enacted by the Society, on the recommendation of the Council, to authorise the award of a joint Medal when it appears expedient to recognize the independent researches of two or more persons who have been working independently on the same subject, and who at about the same time have announced publicly the results of their separate labours. In past years much inconvenience has been experienced for want of such a power on the part of the Council, though on one occasion in 1867 means were found to justify the award of a joint Medal. But in this case the recipients were co-workers, and the Fellows, with enthusiasm, condoned that somewhat irregular act of the Council, which apparently was a departure from a course which the bye-laws had prescribed. On inspecting the new bye-law, enacted in 1871, it will be perceived that the Council are now authorized to receive the nomination of two or more persons as joint recipients of the Medal, provided that the nominees are the simultaneous, but independent, authors of any treatise, work, research, or discovery; and the bye-law goes on to say that "should the medal be ultimately awarded to such joint authors, workers, or discoverers, an impression of the Medal shall be provided for and given to each of such joint recipients." The Council have therefore great pleasure in carrying out, for the first time, the terms of this new bye-law, by their award recognising the important and interesting stellar researches independently carried on by Professors Pritchard and Pickering, as a comparatively new subject of physical inquiry, combining new principles and apparatus in the investigation.

Since the appointment of Professor Pickering to the superintendence of the Observatory of Harvard College, he has devoted much time and attention to the subject of the comparative lustre of the stars, the results of which have from time to time been communicated to astronomers. The magnitude of his work on the photometry of the stars was not, however, realized till the appearance in 1884 of the first part of Vol. XIV. of the *Annals of the Astronomical Observatory of Harvard College*, which includes the results of his photometric examination of all stars of the sixth magnitude to 30° south declination. This great work contains chapters giving a detailed description, with diagrams of the meridian photometer used in the investigation; a summary of the whole series of observations, including a discussion of the estimated magnitudes of *Polaris*, the star to which all other stars were referred; on the atmospheric absorption of light; eye-estimations of the brightness of the stars visible to the unaided eye, for comparison with the corresponding photometric determinations; and a general catalogue giving in tabular form the main results of the entire investigation. The number of stars included in the catalogue is 4,260, representing 700 series of observations, and 94,476 separate comparisons made between October 25th, 1879, and September 17th, 1882. The list contains all the stars not fainter than the sixth magnitude between the North Pole and 30° south declination, or as low as stars can be conveniently observed for this purpose in latitude 42° N. The stars were selected from several standard catalogues, in which the magnitude is given as, at least, the sixth. They have all been observed near the meridian, where they have the greatest altitude, thus facilitating the computation of the correction for atmospheric absorption. A second part of the work appeared in 1885, consisting mainly of a detailed discussion and comparison of all the original star magnitudes found in various original catalogues, from those of Ptolemy and All Sûfi to modern times, and it is a discussion of some magnitude, exhibiting in its compilation the results of great zeal and research. In addition to this comparison, this part includes separate chapters on discordant observations, suspected variables, the distribution of stars, and other subjects. Having described the instruments with which the Harvard photometric observations were made, the President proceeded to describe the work of Professor Pritchard. He said: I believe I am correct in saying that at the commencement of his observations Professor Pritchard was unaware of the methods and intentions of Professor Pickering. We must, therefore, consider the two investigations as strictly independent and original, and as such they are both recognised by the Council in the award of a joint Medal. The *Uranometria Nova Oxoniensis* contains the results of all Professor Pritchard's Observations, made with a wedge photometer of nearly neutral tinted glass 6½in. long, and 1in. broad, and 0·145in. thick, tapering off to 0·02in., cemented to a similar wedge of white glass placed the reverse way. The whole slides in a groove in the brass cap of the eyepiece close to the achromatic eye-lens, and is thus placed between the eye of the observer and the telescope. Usually the position of the wedge when the light of the star is just extinguished is observed five times, and the mean of all the five readings is called the wedge reading. In order to illiminate changes of atmospheric absorption the extinction of Polaris has been observed at the beginning, middle, and end of every series of observations. In this manner the magnitudes of 2,784 stars have been determined. The stars have been selected from the *Uranometria Nova,* ranging from the North Pole to 10° south of the Equator.

It is not my desire or intention to make any critical comparison of the results obtained by the wedge-photometer with those derived from other sources. This will probably be done by others at the proper time, and in a more formal manner. But the question naturally arises, Are the results obtained by the wedge-photometer to be accepted as fairly accurate representations of the relative stellar magnitudes? Professor

Pritchard considers they are, as a whole, sensibly accurate to the tenth of a magnitude; and if the probable error be any indication of accuracy, I think he has reasonable grounds for his belief. We have had lately several important discussions at the meetings of the Society on the capabilities of the wedge-photometer, besides other criticisms. Some of these are valuable, especially those of Dr. Wilsing in the *Astronomische Nachrichten*, No. 2,680, but at the same time I believe that the explanations given by Professor Pritchard in answer to these criticisms have practically smoothed down many of the difficulties pointed out, thus minimising the effect of some of the systematic errors to which this form of photometer may be liable.

On the termination of his address, the President presented one of the medals to Professor Pritchard, and handed the other to Dr. Huggins to forward to Professor Pickering, addressing them respectively in the following terms:—

"Professor Pritchard,—I have very great pleasure in presenting this medal, which the Council have awarded to you conjointly with Professor Pickering. I hope you will accept it as an acknowledgment on their part of the excellent work you have accomplished during your superintendence of the University Observatory at Oxford. I assure you that the important contribution you have made to astronomy on the relative magnitudes of the stars is duly appreciated by the Fellows, and I trust that you may still be spared many years to continue your valuable researches."

"Dr. Huggins,—In forwarding this medal to Professor Pickering, I would request you to inform him that the Council entertain the highest opinion on the merits of the important research on which he has been engaged, and for which they have had much pleasure in awarding to him this medal conjointly with Professor Pritchard. I trust that what he has so successfully accomplished is only the prelude of further researches, which will add still more to the great astronomical reputation he has already achieved."

It was proposed by Professor George Forbes, and seconded by Mr. Brett: "That the report of the Council be adopted and printed, and circulated in the usual manner."

The motion having been put and carried,

The President called upon Mr. Ranyard to move the following resolution, of which he had given notice: "That bye-laws 71 to 75*, both inclusive, referring to the award of the medal of the Society, be repealed."

Mr. Ranyard said: I think I had better explain, at starting, the meaning of the motion, as I find there are some Fellows who have not understood it. It is proposed to repeal the bye-laws which refer to giving the medal; and my meaning is, that no medal should be given in future. I am quite aware that in making such a proposal I am opposing the current opinion that has prevailed hitherto among the members of scientific societies. A medal has been given for more than sixty years by our own Society, nearly from its commencement. The first medal was given in the third year after the Society's birth; and recently a great many new medals have been founded by Institutions and individuals; within the past ten or fifteen years probably the number of medals awarded has been doubled. The proposition I am about to make implies that this has been a mistake, and that it would have been better if no medals had been given. I am quite aware of the responsibility I am undertaking in asking the Society to lead the way in a contrary direction. It has been suggested to me by some of my friends that I should begin by proposing that medals should only be given to foreigners; but, after thinking the matter over, I have decided not to begin with the thin edge of the wedge, but to put the proposition before you as a whole, and to ask you to judge whether it is to the advantage of science that such societies as ours should award medals. I will endeavour to lay before you as fairly as I can the reasons which may be suggested for giving medals, and I should like to hear any other reasons which can be given to justify the making of such awards, for I think that it lies with those who approve of medal-giving to show that medals tend to advance science, or that they are for the benefit of such a society as ours. (Cries of "No, no.") In the first place, it may be said that the giving of medals affords an opportunity of discussing the work of contemporary workers. There is something to be learnt from such discussion when it is fairly and generously conducted; but in this instance the discussion does not take place before the body of the Society—the discussion takes place in the Council. Three meetings of the Council are practically devoted to these discussions every year, and the question is whether it is worth while, for the sake of instructing the members of the Council, to require them to devote three meetings every year to such work. There are plenty of other opportunities of discussing the work of contemporaries and co-workers before the body of the Society. I think it can hardly be argued that it is worth while to retain the medal for the sake of such discussions in the Council. Then it is said that such medals do honour to those who have accomplished important work in astronomy. (Hear, hear.) Perhaps they do—(laughter)

—but the honour is done them at the time when they do not want honour. (Cries of "Oh.")

Professor Pritchard: When is that?

Mr. Ranyard: When they have achieved it. Their work, if properly done, brings its own reward. In the long run they will have to stand by their work and be judged by it, and it will be looked upon as quite an incident whether they received a medal or not. It is by his work that a man is remembered in after time. (A voice: He is dead then.) I am discussing the effect on the worker during his life, as well as on the history of science. It is by his work a man is remembered, and I think that perhaps it would be better that during his life he should feel that he has to stand upon his work, for there is a tendency in human nature, as we all know, to rest upon our oars when honours have been won, and as to the rest of the scientific world, I think it can be shown that they would be in a better position to do good work if they did not attempt to distribute medals. Then it is said that medals are a great incentive to work. (Hear, hear.) Now, I question whether any gentleman who has received a medal —and there are a good many medallists here—would say that he did the work for the medal, or that he had it in view when he commenced the work. I expect that very different motives impel a man to do good work. There must be enthusiasm for it, and the medal only comes in as an incident afterwards. I should be very much surprised if any medallist here were to stand up and say that he did his work in order to get the medal. (A Voice: Not one of them.) Then I have the meeting with me in this—that medals are not an incentive to work. I shall be glad to hear what the medallists present say about the effect of medals on their work. I do not for a moment wish to suggest that they did anything wrong in accepting medals—(Laughter)—for they no doubt did not think as I do, or perhaps they did not think about it. (Laughter.) This is a serious question, and ought not to be met by laughter. I think that I have the meeting with me, that medals are not an incentive to work. (Yes.) Well, then it is said that medals form a very suitable way of commemorating a great man: but who knows anything about Copley the Baronet, in whose name the Royal Society medal has been given for a century and a half? I question whether anyone in this room knows anything about him; but our medal is not given in commemoration of anyone, so this cannot be given as a reason for keeping up the Astronomical Society's medal, and it does not make much difference to such people after a lengthened time.

Professor Pritchard: I think it does.

Mr. Ranyard: I think it would be better that such men should rest on their own merits, and that no attempt should be made to advertise them in any extraneous manner. Then it is said that the giving of medals is useful, because it leads the way in astronomy; but I question whether anyone looking at the list of the Copley medallists will think that the list at all marks out the history of science. I do not wish to go into instances, or to refer to the list of our own medallists, because I wish to avoid anything that might give rise to a discussion of personal merits; but I think that anyone looking through any long list of medallists will come to the conclusion that it does not satisfactorily mark out the history of the science with which the medals are connected. Many of the Copley medallists are quite unknown names; and where a medal has been awarded to a person who is shown by later experience to be unworthy, it has led, or has tended to lead the community wrong at the time.

Mr. Howlett: Do your remarks refer only to the Royal Society medal, or do you make the same suggestion with regard to our own?

Mr. Ranyard: I took the Royal Society medal as an instance, in order to avoid anything which might lead to a personal discussion, and because the list of medallists extends back into the last century, and we are better able to judge fairly after a lapse of time; but my remarks apply to all long lists of medals. No guide is better than a bad guide, and if such medals have not picked out and marked the best men, we had better have been without them. Efforts have been made recently in the newspapers to point out the hundred best books for people to read—(laughter)—but it seems to be pretty generally agreed that such efforts have not been very successful, and if the best books cannot be picked out with advantage, how can the best workers be picked out by their contemporaries; each man must judge for himself with regard to the books he will read. It is part of our education to make such judgments for ourselves, and it is better that we should not fall into a stereotyped plan of following others. With regard to giving medals to specialists, no council, however able, will be able to judge justly of the work of specialists; they usually rely on the estimation of other specialists working in the same line, and their consciences are satisfied, but any committee puts itself in a false position if it attempts to sit in judgment on specialists. The members of the committee must either rely entirely on the judgment of a few specialists in the same line, or they must pretend to give awards on their own responsibility outside their own line. I think we had better avoid all such false positions. The awarding of medals always necessarily involves personal questions with regard to comtemporaries, and sometimes with regard to co-workers with whom those who award the medals are associated, and such discussions always have a tendency to lower the scientific spirit. (Cries of "No.") It is a matter of the greatest importance, for the scientific spirit is a very delicate plant; and its healthy growth deserves the closest attention. We ought to avoid anything that may injure the scientific *morale.* (Cries of "No, no.")

Mr. Esdaile: I beg to second the motion.

Mr. De la Rue: I stopped here specially to hear Mr. Ranyard's proposal, and to hear his reasons for depriving the Society of a means of gracefully recognizing the merits of astronomers, and I must say I have not heard one argument which can in any degree, in my opinion, justify us in depriving ourselves of this very great privilege. He is perfectly right in saying that the medals do not tend to promote work for the sake of obtaining them. No man who has ever received a medal, I am quite sure, had the medal in his mind when he commenced his work. The work is done simply for the love of it. (Hear, hear.) A great deal of labour is gone through and self-sacrifice made, for the sake of the work, but not for any possible reward. I think it would be a very great pity, even in these revolutionary times (laughter), to do away with one of the grandest privileges that this Society possesses.

Mr. Ranyard: I wish some gentleman would try and meet the particular arguments I have used (laughter). To say that it is a privilege to give medals is not meeting the arguments I have brought forward as to their influence.

Mr. De la Rue: It is very difficult to meet a shadow.

Mr. Ranyard: I have brought forward some arguments, and I should like to hear something said by those who believe in medals. [Cries of "Vote, vote."]

The President: I will take that as your reply, Mr. Ranyard. I will now put the resolution.

On a show of hands there was a decided majority against the resolution, a considerable minority abstaining from voting, only three hands being held up in favour of the motion.

The scrutineers declared the following list of council and officers to have been elected for the ensuing year:—

President.

J. W. L. Glaisher, Esq., M.A., F.R.S.

Vice-Presidents.

J. C. Adams, Esq., M.A., LL.D., D.C.L., F.R.S., Lowndean Professor of Astronomy, Cambridge.
W. H. M. Christie, Esq., M.A., F.R.S., Astronomer Royal.
The Earl of Crawford and Balcarres, F.R.S.
Edwin Dunkin, Esq., F.R.S.

Treasurer.

A. A. Common, Esq., F.R.S.

Secretaries.

E. B. Knobel, Esq.
Lieut.-Col. G. L. Tupman, R.M.A.

Foreign Secretary.

William Huggins, Esq., LL.D., D.C.L., F.R.S.

Council.

J. Rand Capron, Esq.
Arthur Cayley, Esq., M.A., LL.D., D.C.L., F.R.S., Sadlerian Professor of Pure Mathematics, Cambridge.
G. F. Chambers, Esq.
A. M. W. Downing, Esq., M.A.
J. R. Hind, Esq., LL.D., F.R.S., Superintendent of the "Nautical Almanac."
George Knott, Esq., LL.B.
E. W. Maunder, Esq.
Captain William Noble.
Rev. Charles Pritchard, D.D., F.R.S., Savilian Professor of Astronomy, Oxford.
A. Cowper Ranyard, Esq., M.A.
E. J. Stone, Esq., M.A., F.R.S., Radcliffe Observer.
Lieut.-Gen. J. F. Tennant, R.E., F.R.S.

SELENOGRAPHICAL NOTES, MARCH, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Inghirami. This large but very foreshortened ring-plain, from its position with respect to the limb, is under no conditions very favourably placed for observation, though much can be distinguished in the form of long parallel ridges, crater-rows, and other features to interest the observer. On September 22, 1885, a cleft was traced from a small crater just beyond the southern border of Inghirami to the more northerly of a pair of large craters some little distance to the south-east of Wargentin. It runs from north to south and exhibits many local widenings and contractions. The northern end of Mädler's rill-valley δ, which is marked by a pair of minute craters, is slightly to the south-west of the southern end of this cleft, and from the trend of the latter it seems probable that the two objects are connected. Schmidt shows merely a ridge or two on the site of this object. West of the cleft, and running closely parallel to it, is a long steep ridge, which under a high angle of illumination might be mistaken for a second cleft. On the floor of Inghirami Mädler and Neison show two craters, one near the north and the other adjoining the south border, and a long ridge running concentrically with the western border, while Schmidt (Sect. xxi.) draws a short crater-row near the north-west side of the interior, three good-sized craters west of the centre, and a somewhat complicated arrangement of ridges on the eastern half of the floor. I was unable to identify any of these objects. On the northern part of the interior two round black spots—probably crater-pits—were remarked, and a very conspicuous ridge, originating at the inner foot of the south-east wall, to which it runs parallel for some distance, and then, turning suddenly towards the north-west, strikes across the floor to the opposite border, swelling near the centre into a considerable boss, almost worthy of being classed as a central mountain. Schmidt shows three parallel ridges on the north of Inghirami, the most easterly of which is continued through the border and penetrates some distance into the interior. The others are interrupted by the wall and by two crateriform objects just within it, but appear again on the floor. I was able to trace these features up to the border, but no further; the only object visible on the northern part of the interior, in addition to the two dark spots just mentioned, being a short cleft running from the inner foot of the north-east wall towards the centre. There is a considerable ring-plain north-west of Inghirami (*a*, *Neison*, *Map XVI.*), which, according to Mädler and Neison (though the latter does not show it), has a central mountain. Schmidt apparently overlooked it. It undoubtedly exists, but is by no means obvious at any time.

Region West of Copernicus. The lunar section of the Liverpool Astronomical Society has undertaken the preparation of a special chart and monograph of the very interesting district included between N. lat. 0°—12° and E. long. 10°—17°, or areas II A *γ*, II A *θ*, and portions of II A *δ*, II A *ι*, II A *ν*, II A *ξ*. The scale adopted is nearly double that of Schmidt's Map, so as to allow the smallest details visible to be accurately inserted. In spite of the wretched observing weather that has prevailed during the past three lunations, fair progress has been made with portions of areas II A *θ* and II A *ν*, which includes Stadius and the well-known crater-row north-west of Copernicus. More than 60 considerable craters and craterlets have been observed, many of them hitherto unrecorded, on and about the site of Stadius, the most remarkable being a group of 12 immediately surrounding

Stadius B. The principal object of the section in making this organized attempt to advance selenography is to chart the very remarkable region lying between Gambart and Eratosthenes, a large portion of which is utterly unlike anything we find elsewhere on the moon's visible surface, including, as it does, the enigmatical dark grey spots detected by Schmidt in April, 1873, and recently described by Dr. Klein. No existing map is on a sufficiently large scale to show properly the numberless little hills with which these features are associated, or to give a just idea of the true character of the region in the neighbourhood of N. lat. 5° and E. long. 15°. At present there are some eight or nine volunteers engaged in this work, but more observers are needed. Will some of your numerous readers possessing large telescopes give, at any rate, occasional assistance?

Kempston, Beds: February 20.

On the VARIATION IN COLOUR of the NOVA Andromeda; and the evidence it affords as to the origin of the NOVA.

By B. J. Hopkins.

This remarkable star, if star it can be called—*starlike object* being perhaps a more accurate designation—is beyond doubt one of the most mysterious phenomena that has ever attracted the attention of astronomers. Its sudden apparition, together with its complete and rapid extinction, make the phenomena presented by it difficult of explanation. Though faint and uncertain traces of two or three bright lines were perceived in its spectrum, they were too dubious to admit any measurement; so that it is impossible to class it with certainty among "temporary" stars of the type of T Coronæ. It may perhaps be explainable upon the supposition that it was a previously invisible long period variable star, rendered apparent through having attained its maximum brightness; but in that case, its not having been observed before would give it a period longer than any known variable; and would also differ from such of those bodies with which we are acquainted in that it changed colour as it decreased in magnitude. It is true this "star" may have been one by itself in that respect; but whether such is the case or not is a question for future generations to decide.

But even granting that it was a "temporary" star, or a long period variable, we only half solve the enigma presented to us by this star and other of a similar class, for the question still remains—What is the cause of these bodies appearing?

Now perhaps the best answer to this question, and one which appears to offer a feasible solution of the problem presented to us by the appearance of this star in particular, is to be found in a very ingenious hypothesis recently put forward by Professor Monck, of Dublin; whether the hypothesis will account for the appearance of *all* the "new" stars that have from time to time been observed, I am not prepared to say; but considered in relation to the phenomena presented by the *Nova* forming the subject of the present paper, it certainly appears to me to have an air of probability about it, and in fact may be regarded as the best suggested explanation of the cause of "temporary" stars. It is not necessary for me to quote here the whole of the arguments put forward by Professor Monck in support of his hypothesis, as they are to be found in a letter to the "Observatory," Vol. viii, p. 335; I shall therefore merely content myself by stating in as few words as possible what that hypothesis is, and as briefly as possible give my reasons for regarding it as offering the best explanation of the variation in the Andromeda nebula.

The hypothesis, then, may be briefly stated as follows:—a star previously dark (if such a phase may be used), or but faintly luminous, might acquire a short-lived brilliancy by rushing through a gaseous nebula; and also that a nebula through which such a star passed would be rendered more luminous.

Such, in short, is the hypothesis as I understand it, and the question remains—What should we expect to see in such a case; and did the phenomena exhibited by the *Nova* agree with that which we should expect? Respecting the first part of this query, we should naturally expect the particles of matter of which the star or nebula was composed, to behave similarly to terrestial substances when heated viz.: to first emit light of low refrangibility, and as the temperature increased the more refrangible rays would make their appearance in the order of their wave lengths.

And it is interesting to know that the variations observed in the colour of the *Nova* were just what we might expect to witness, if the temperature of the particles of a nebula were increased by the passage of a star through it; for the predominating colour of the light was red when the *Nova* was first observed; it then passed to orange; then to yellow, and finally the predominating colour was a bluish-white, at which it remained until it faded from view.

This change in the colour of the "new" star—to which I was the first to direct attention in the *English Mechanic,* Vol. xlii., p. 99—having been questioned by one or two observers of no mean ability, I have thought it advisable to conclude these few remarks with the particulars of the few observations I was able to make upon it, supplemented by those observations of other observers that have come under my notice.

Mr. Hopkins' observations of the Colour of the Nova Andromeda.

1885.
Sept. 3. Observed the new star in the great nebula in Andromeda, information of the discovery of which I received yesterday in Dun Echt Circular, 97. "It was of a *yellowish* colour."
„ 9. 10h. "Could discern no difference excepting perhaps it was not quite so decidedly *yellow*."
„ 13. 9h. 30m. "It certainly was not so *yellow*."
„ 27. 8h. "It has changed colour, appearing to my eyes of a *bluish* hue."
„ 16. 9h. 45m. "Decidedly *bluish* in colour."
„ 29. "It was not now so blue, being tinged with white."

As these observations of mine were made with a refractor of only 3 in. aperture and powers of 50 and 100, they would not perhaps be considered sufficiently trustworthy by themselves to prove the change in colour; I therefore supplement with the following

Observations by various observers of the colour of the Nova Andromeda (arranged according to date).

1885.
Sept. 3. "It presented the appearance of an *orange coloured* star."—Dr. W. Huggins, Tulse Hill, Brixton, S.W. *Nature*, Vol. xxxii.
„ 3. "The nucleus appearing as an intensely bright point resembling a 7 mag. *yellow* star."—H. Michell Whitley, Westminster. *Observatory*, Vol. viii.
„ 4. "The colour of the star is *reddish yellow*."—Dr. N. de Konkoly, O'Gyallai. *Observatory*, Vol. viii.
„ 4. "It was . . . . *orange-red*, and hazy with high powers."—Kenneth J. Tarrant, Pinner. *Eng. Mec.*, Vol. xlii.
„ 5. "A well defined *orange-coloured* star."—C. E. Peek, Lyme Regis. *Eng. Mec.*, Vol. xlii.
„ 7. "The colour of the stellar point appeared much the same as that of Aldebaran."—Rosse, Parsonstown. *Nature*, Vol. xxxii.
„ 13. "The nova shines with a *yellowish* tint." F. Denning, Bristol. "*Nature*, Vol. xxxii.

Sept. 14. "The nebula was examined here on the 5th and 9th, and the star was very conspicuous. Two observers thought it *bluish* in tint. J. Rand Capron, Guildown. *Eng. Mec.* Vol. xlii.

Oct. 12. "The nova was . . . . on the 10th inst. of a *bluish* cast, whilst to me it appeared to be *yellowish* when at its brightest early in September."—J. W. Ward, Belfast." *Eng. Mec.*, Vol. xlii.

" 16. "When last observed on the 7th inst. . . . . it was *bluish.*"—Kenneth J. Tarrant, Pinner. *Eng. Mec.* Vol. xlii.

At first sight the foregoing observations appear very contradictory, but an examination will at once show that they closely agree with my observations—especially when the smallness of my instrument is taken into account—and also that the evidence is very strong indeed that the star was at first of a *yellowish* tint, and after an interval of a few days gave forth a *bluish-white* light; that in fact the star did actually undergo a change in colour. Every unprejudiced reader will, I presume, admit that much; and considering that several observers noted the star of a reddish colour when it first made its appearance, and that it was later on seen by several more observers of an orange colour, and then yellowish, and finally bluish, they will I think agree with me that the explanation of its origin I have endeavoured to make clear in this paper, is to say the least a probable one.

Forest Gate, E. B. J. H.

NOTES ON SOME STARS IN THE URANOMETRIA NOVA OXONIENSIS.

By J. E. Gore, F.R.A.S.

The following is a list of stars in the *Uranometria Nova Oxoniensis,* the magnitudes of which, as measured at Oxford, differ from the measures made at Harvard by half a magnitude or more. As the Oxford standard is Polaris =2·05m., and the Harvard standard Polaris =2·15m., I have subtracted 0·1m. from the Harvard measures. Some of the stars have been suspected of variation,

Star.	R.A. 1890·0.	Decl. 1890·0.	Oxford Mag.	H.P. Mag. — 0·10.
	h. m.	° ′		
(1) P. O., 118 Cassiopeiæ	0 30·0	+ 53 34	5·48	4·90
(2) ε Cephei	22 11·0	+ 56 30	4·76	4·14
(3) 22 Cygni	19 51·9	+ 38 12	5·14	4·57
(4) 30 Geminorum ...	6 37·8	+ 13 20	5·19	4·52
(5) λ Herculis	17 26·3	+ 26 12	4·70	4·19
(6) W. B. xvii., 1438 Herculis ...	17 46·0	+ 29 22	6·17	5·56
(7) 1 Lacertæ	22 11·2	+ 37 12	4·64	4·04
(8) 2 Lyncis	6 9·9	+ 59 3	4·73	4·23
(9) 8 Monocerotis ...	6 17·9	+ 4 39	4·89	4·30
(10) π^2 Orionis	4 44·6	+ 8 43	4·86	4·32
(11) υ Orionis	5 26·6	— 7 23	5·18	4·56
(12) 42 Orionis	5 30·0	— 4 55	5·45	4·50
(13) 45 Orionis	5 30·2	— 4 56	5·78	4·85
(14) χ^1 Orionis	5 47·9	+ 20 15	5·06	4·55
(15) μ Orionis	5 56·3	+ 9 39	4·69	4·19
(16) ρ Persei	2 58·1	+ 38 25	4·24	3·58
(17) ω Tauri	4 10·8	+ 20 19	5·08	4·55
(18) 71 Tauri	4 20·1	+ 15 22	5·03	4·51
(19) 5 Ursæ Minoris ...	14 27·7	+ 76 11	4·72	4·19
(20) P. xiv., 12 Virginis	14 6·7	+ 2 56	5·44	4·87
(21) 110 Virginis ...	14 57·3	+ 2 33	4·99	4·46
(22) 12 Vulpeculæ ...	19 46·3	+ 22 20	5·49	4·92

NOTES ON THE ABOVE.

(1) P. O., 118 Cassiopeiæ = B. A. C. 146, 6m. Argelander and Heis.

(2) ε Cephei. 5–4 Argelander and Heis.

(3) 22 Cygni. 5–6 Argelander and Heis.

(4) 30 Geminorum. Estimated 5·2 at Cordoba.

(5) λ Herculis. 5m. Argelander, 5–4 Heis.

(6) W. B. xvii., 1438 Herculis = D. M. + 29°, 3126, 6m. Argelander, 6–5 Heis.

(7) 1 Lacertæ. 5–4 Argelander and Heis.

(8) 2 Lyncis. 5–4 Argelander and Heis.

(9) 8 Monocerotis. 5–4 Argelander and Heis, and 4·8 at Cordoba.

(10) π^2 Orionis (Fl. 2). 5–4 Argelander and Heis, and 4·7 at Cordoba.

(11) υ Orionis. 5–4 Argelander, 5 Heis, and 5·1 at Cordoba.

(12) 42 Orionis. 4m. Al-Sufi, 6m. Lalande (10540), 5·5 at Cordoba. Dr. Gould suspects variation in either 42 or 45.

(13) 45 Orionis. 7m. Lalande (10555). 42 and 45 were together rated 5–4 by Argelander and Heis. Between the two is an 8m. star, Lalande (10547).

(14) χ^1 Orionis. 5–4 Argelander and Heis, and 5·0 in D. M.

(15) μ Orionis. 5–4 Argelander and Heis, and 4·7 at Cordoba.

(16) ρ Persei. A known variable. Irregular. Sawyer found its light nearly constant during the year 1884.

(17) ω Tauri. 6–5 Argelander and Heis, or much fainter than the measured magnitudes.

(18) 71 Tauri. 6 Argelander and Heis, and 5·0 D. M. Although measured 4·61 at Harvard, it was "estimated" 5·5m.

(19) 5 Ursæ Minoris. 5–4 Argelander and Heis, and 5·0 in D. M.

(20) P xiv., 12 Virginis = Lalande 26031, 5–4 Argelander and Heis. The Cordoba estimates vary from 4·8 to 5·5, and Dr. Gould thinks "the discordance is altogether too large to be attributed to errors of observation" (*U. Argentina*, p. 320). On April 7, 1884, I estimated this star 5·1m.

(21) 110 Virginis. 5m. Argelander and Heis, and 4·9 at Cordoba.

(22) 12 Vulpeculæ. 5m. Argelander, 6m. Heis, and 5·7 D. M. In December, 1876, I found it brighter than 14, and about equal to 13 Vulpeculæ.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

GALILEO'S RANK IN THE MOON.

Sir,—My proposal for allotting to Galileo a fitting representative, was especially designed to avoid the very objections which Mr. Elger sees to it, and which stand, as we all know, in the way of a general reform of names in the moon. I do not propose any such reform, nor to "shift" the present *Galileo*, but to retain it.

And this would seem to be justified, and would explain itself, from the case being so exceptional, as not, in my knowledge, to have any parallel.

Also, apart from Galileo's claims, the absence of a name to the little sea in question is sure to be supplied sooner or later. It is inconsistent: we have other portions—bays, &c.—of the grey plains, with less individuality than this one, distinguished by names. And it is inconvenient: for my own purposes, in my observational notes, I have long used a name for this enclosure, though not the one I now suggest, to save the circumlocution of repeated description.

EDWARD ALLEN, M.A., F.R.A.S.

Castlechurch Vicarage: February 2, 1886.

PROPOSED MAPS FOR TRACING METEOR PATHS.

Dear Sir,—As Mr. Backhouse has asked for suggestions, preparatory to issuing instructions for the construction of star maps for the use of meteor observers, I need not hesitate to set down here some views which I have long held as to maps in general; the less so, as they seem peculiarly applicable to maps of stars. My point of view is this: That a map is primarily a *chart*, and its purpose to indicate *locality*, and what depends immediately on locality—feature and character. All descriptive detail, conveyed otherwise than through technical signs, or by the use of colour, is destructive of the desired simplicity, through introducing matter foreign to the primitive conception of a chart. But, inasmuch as a map would be of comparatively little value which conveyed no information, except as to position and nature, I would have it always accompanied by a copy, which should serve as its explanation, and on which might be put much more descriptive detail than can ever be permitted on a map as at present constructed, in which it is absolutely necessary to have some regard to the primary purpose.

In the case of star charts, I suppose few persons who have used them have not experienced the sense of obstruction to a correct mental picture of the configurations, caused by the laudable, but utterly arbitrary, attempts to guide the eye, and instruct the mind, by means of various lines having no counterparts in the sky. Some of these would be very useful if placed on a companion chart; and others, it may be admitted, are absolutely necessary to give the chart its full measure of utility. But they are all, nevertheless, *de trop*, where the immediate object is recognition of certain stars seen in the heavens upon a chart purporting to represent them all.

I suggest, therefore, that for the use of meteor observers, what is especially wanted is, numerous copies of a simple star chart, *without lines or letterpress of any sort;* accompanied by a single index-sheet, on which all desirable information of the ordinary sort should be superposed upon the simpler one, preferably in a different colour.

As I should probably spoil my suggestion, or at least its chance of acceptance, by proceeding to advocate it, I prefer to leave it, in its naked beauty and simplicity, to commend itself. I will only press one point. Observers of a thrifty and careful turn naturally hesitate to spoil a full-blown map, who would have less scruple about using—for its intended purpose—the comparatively blank chart; on which lines might be pencilled *ad libitum*, and rubbed out without fear, until a fresh sheet might seem desirable. These blank charts would, of course, be very much cheaper than the maps.

I am, yours obediently,

J. HERSCHEL, Lieut.-Col. R.E.

Collingwood : February 23.

REVIEWS.

Uranometria Nova Oxoniensis. A photographic determination of the magnitudes of all stars visible to the naked eye, from the Pole to ten degrees south of the Equator. By C. Pritchard, D.D., F.R.S., F.R.A.S., Savilian Professor of Astronomy in Oxford. Oxford, at the Clarendon Press. 1885. pp. xxvii., 117. Price 8s. 6d.

The introduction contains a general historical survey of astrometry, instrumental aids to sidereal photometry, description and use of the wedge photometer, with formulæ and tables, observations taken at Cairo, and explanation of the catalogue.

Observing that the usual scale of magnitudes for stars exceeding the 1st mag. by 0 and —, leads to confusion, Professor Pritchard proposes that the symbol + placed before a magnitude shall indicate that the numerical expression means so many magnitudes or portions of a magnitude brighter than the first; so that + 0·4 and + 1·3 shall indicate respectively, four-tenths of a magnitude, and one and three-tenths of a magnitude, brighter than the first. In this way the magnitude of Sirius is designated by + 1·95, indicating that it is very nearly two magnitudes brighter than a star, such as α Aquilæ or Spica, which are very properly regarded as approximately of the first magnitude on Pogson's scale. The following table gives the photometric magnitude of such stars as have heretofore been grouped together as of the first order of lustre :—

Star's name.	Magnitude.	Star's name.	Magnitude.
Sirius	+ 1·95	Spica	+ 0·04
Rigel	+ 1·03	α Orionis	+ 0·02
Capella	+ 0·92	α Aquilæ	1·04
Vega	+ 0·86	Aldebaran	1·12
Arcturus	+ 0.69	Antares	1·13
Procyon	+ 0·50	Pollux	1·36

Some will be surprised at the *degradation* of Sirius, which Sir John Herschel estimated to be four times as bright as *α* Centauri; so Seidel, quoted by Newcomb (*Popular Astronomy*). In the Harvard photometry + 2·33 is assigned to Sirius.

The fruit of much toil and skill, this work of the Savilian Professor will doubtless become a standard one. He records how much he is indebted to the skilful and unremitting attention of his assistants, Mr. W. Plummer, F.R.A.S., and Mr. C. Jenkins, F.R.A.S., by whom the necessary measurements, exceeding seventy thousand, were made.

THE PLANETS FOR MARCH.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	23 7 47	S. 7 8	5″·0	0 30·8
	9th	0 2 43	N. 0 3½	5″·4	0 54·2
	17th	0 50 30	N. 6 58½	6″·6	1 10·4
	25th	1 19 19	N.11 29½	8″·2	1 7·6
Venus ...	1st	21 34 1	S. 5 45½	56″·0	22 53·4
	9th	21 29 19	S. 7 21	50″·3	22 17·3
	17th	21 34 33	S. 8 24	44″·3	21 51·0
	25th	21 48 3	S. 8 46	38″·8	21 33·0
Mars ...	1st	11 21 4	N. 8 40½	16″·4	12 42·1
	9th	11 9 16	N. 9 48½	16″·6	11 58·9
	17th	10 57 43	N.10 47	16″·4	11 15·9
	25th	10 47 45	N.11 29½	15″·9	10 34·5
Jupiter ...	1st	12 15 26	N. 0 1½	40″·8	13 36·3
	9th	12 12 0	N. 0 25	41″·2	13 1·5
	17th	12 8 19	N. 0 49½	41″·4	12 26·4
	25th	12 4 33	N. 1 14	41″·4	11 51·1
Saturn ...	1st	6 6 15	N.22 45	17″·3	7 28·2
	9th	6 6 25	N.22 46½	17″·0	6 56·9
	17th	6 7 6	N.22 48	16″·8	6 26·1
	25th	6 8 17	N.22 49	16″·5	5 55·8
Uranus ...	2nd	12 25 38	S. 1 58	4″·0	13 42·6
	18th	12 23 15	S. 1 41½	4″·0	12 37·3
Neptune ...	2nd	3 24 16	N.16 53		4 42·7
	10th	3 24 51	N.16 55½		4 11·8

Mercury sets twenty minutes after the sun at the beginning of the month, the interval increasing.

Venus rises about an hour and a half before the sun at the beginning of the month, the interval increasing.

Mars rises about a quarter of an hour after sunset on the 1st, and afterwards earlier each night.

Jupiter rises an hour and three quarters after sunset at the beginning of the month, the interval decreasing.

Saturn sets about three hours and a half after midnight on the 1st, the interval decreasing,

DUN ECHT CIRCULAR, No. III.

Elements and Ephemeris of Comet "Fabry."

The elements are computed from observations on December 2, 26, and January 22, 31.

Elements.

T = 1886, April 5·53980 Berlin M. T.
$\pi - \Omega$ = 126° 50′ 27″·6 }
Ω = 36 19 54 ·0 } Mean Equinox 1886·0
i = 82 11 15 ·0 }
log q = 9·804021

Ephemeris for Berlin Midnight.

1886.		R. A.			Decl.			log Δ.	log. r
		h.	m.	s.		°	′		
March	3	23	20	9	N.	30	11·7	0·1787	9·9707
	4	23	20	1		30	28·4		
	5	23	19	52		30	45·3		
	6	23	19	43		31	2·4		
	7	23	19	34		31	19·6	0·1621	9·9441
	8	23	19	24		31	36·9		
	9	23	19	14		31	54·4		
	10	23	19	4		32	12·0		
	11	23	18	54		32	29·8	0·1424	9·9171
	12	23	18	44		32	47·7		
	13	23	18	33		33	5·7		
	14	23	18	22		33	23·8		
	15	23	18	11		33	42·0	0·1191	9·8904
	16	23	18	0		34	0·2		
	17	23	17	49		34	18·4		
	18	23	17	38		34	36·5		
	19	23	17	28		34	54·6	0·0916	9·8650
	20	23	17	19		35	12·6		
	21	23	17	10		35	30·4		
	22	23	17	3		35	48·1		
	23	23	16	57		36	5·6	0·0589	9·8421
	24	23	16	53		36	22·8		
	25	23	16	51		36	39·7		
	26	23	16	52		36	56·3		
	27	23	16	57		37	12·4	0·0203	9·8233
	28	23	17	5		37	28·0		
	29	23	17	17		37	43·1		
	30	23	17	35		37	57·6		
	31	23	17	59	N.	38	11·5	9·9744	9·8102

Dr. H. OPPENHEIM.

Berlin, 1886, February 15.

Lord Crawford's Observatory,
Dun Echt: 1886, February 22.

DUN ECHT CIRCULAR, No. 112.

Ephemeris of Comet "Fabry" continued from *Dun Echt Circular, No.* 111.

Ephemeris for Berlin Midnight.

1886.		R. A.			Decl.			log. Δ	log. r
		h.	m.	s.		°	′		
March	31	23	17	59	N.	38	11·5	9·9744	9·8102
April	1	23	18	30		38	24·6		
	2	23	19	8		38	37·0		
	3	23	19	56		38	48·5		
	4	23	20	55		38	58·9	9·9198	9·8043
	5	23	22	5		39	8·3		
	6	23	23	29		39	16·5		
	7	23	25	8		39	23·4		
	8	23	27	4		39	28·7	9·8547	9·8062
	9	23	29	20		39	32·4		
	10	23	31	58		39	34·2		
	11	23	35	1		39	34·0		
	12	23	38	33		39	31·4	9·7767	9·8157
	13	23	42	36		39	26·0		
	14	23	47	17		39	17·2		
	15	23	52	39		39	4·3		
	16	23	58	48		38	46·9	9·6825	9·8317
	17	0	5	45		38	23·8	9·6560	
	18	0	13	47		37	53·4	9·6282	9·8417
	19	0	22	58		37	14·1	9·5991	
	20	0	33	27		36	24·0	9·5686	9·8527
	21	0	45	24		30	20·0	9·5370	
	22	0	58	58		33	58·3	9·5043	9·8645
	23	1	14	21		32	14·3	9·4708	
	24	1	31	39		30	2·6	9·4370	9·8770
	25	1	50	56		27	17·1	9·4039	
	26	2	12	8		23	52·1	9·3726	9·8899
	27	2	35	5		19	43·7	9·3449	
	28	2	59	25		14	52·2	9·3229	9·9031
	29	3	24	36		9	25·0	9·3088	
	30	3	50	0	N.	3	37·0	9.3042	9·9165
May	1	4	14	56	S.	2	12·2	9·3095	
	2	4	38	50		7	43·5	9·3239	9·9300
	3	5	1	16		12	42·7	9·3457	
	4	5	21	57		17	2·7	9·3727	9·9435
	5	5	40	46		20	42·9	9·4029	
	6	5	57	45	S.	23	46·5	9·4356	9·9569

The comet's greatest brightness will be 460 on April 29, assuming the brightness on December 2 as unity.

Dr. H. OPPENHEIM.

Berlin, 1886, February 15.

Lord Crawford's Observatory,
Dun Echt: 1886, February 22.

ASTRONOMICAL OCCURRENCES FOR MARCH, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Mon	1		Sidereal Time at Mean Noon 22h. 36m. 51·44s.	2nd Ec. D. 1st Ec. D. 2nd Oc. R. 1st Oc. R.	10 14 7 12 8 28 13 56 14 49	12 42·1
Tues	2		Sun's Meridian Passage 12m. 17·66s. after Mean Noon	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	9 19 9 4 6 11 35 12 1	12 36·8
Wed	3	10	Conjunction of Moon and Venus 6° 10′ N.	2nd Tr. E. 2nd Sh. E.	8 35 9 15	12 31·4
Thur	4		Saturn's Ring: Major axis=43″·23 Minor axis=19″·39			12 26·0
Fri	5	10 4	● New Moon Eclipse of the Sun, invisible at Greenwich			12 20·6
Sat	6	0 6	Mars at opposition to the Sun Conjunction of Moon and Mercury 0° 8′ N.	2nd Sh. I. 3rd Tr. I. 2nd Sh. I.	15 55 17 29 18 13	12 15·1
Sun	7			1st Sh. I. 1st Tr. I.	16 44 17 4	12 9·7
Mon	8	6 11	Near approach of B.A.C. 408 (6½)	2nd Ec. D. 1st Ec. D. 2nd Oc. R. 1st Oc. R.	12 50 30 14 1 55 16 13 16 33	12 4·3
Tues	9	5 31 6 38 6 35 7 39	Occultation of 64 Ceti (6) Reappearance of ditto Occultation of ξ¹ Ceti (4½) Reappearance of ditto	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	11 13 11 31 13 29 13 45	11 58·9
Wed	10			2nd Sh. I. 2nd Tr. I. 1st Ec. D. 3rd Oc. R. 2nd Sh. E. 2nd Tr. E. 1st Oc. R.	7 30 8 4 8 30 17 9 58 10 18 10 49 10 59	11 53·5
Thur	11			1st Sh. E. 1st Tr. E. 4th Ec. D. 4th Ec. R.	7 58 8 11 10 45 2 12 28 45	11 48·1
Fri	12					11 42·7
Sat	13	1 17 4 49 6 1 12 40 13 28 16	☽ Moon's First Quarter Occultation of 130 Tauri (6) Reappearance of ditto Occultation of B.A.C. 1930 (6½) Reappearance of ditto Conjunction of Moon and Saturn 4° 27′ N.			11 37·3
Sun	14					11 31·9
Mon	15	10 15 11 12	Occultation of 1 Cancri (6) Reappearance of ditto Illuminated portion of disc of Venus=0·163 Illuminated portion of disc of Mars=0·996	2nd Ec. D. 1st Ec. D.	15 29 15 55 28	11 26·6

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars. —
Tues	16		Sidereal Time at Mean Noon 23h. 35m. 59·72s.	**1st** Sh. I. **1st** Tr. I. **1st** Sh. E. **1st** Tr. E.	13 7 13 15 15 23 15 29	11 21·3
Wed	17		Sun's Meridian Passage 8m. 27·32s. after Mean Noon	**2nd** Sh. I. **3rd** Ec. D. **2nd** Tr. I. **1st** Ec. D. **1st** Oc. R. **2nd** Sh. E. **2nd** Tr. E. **3rd** Oc. R.	10 3 10 6 43 10 16 10 23 52 12 42 12 52 13 1 13 14	11 16·9
Thur	18	5 25 6 17 14	Occultation of 37 Sextantis (6) Reappearance of ditto Conjunction of Moon and Mars 4° 27′ N.	**1st** Sh. I. **1st** Tr. I. **1st** Sh. E. **1st** Tr. E.	7 36 7 40 9 52 9 55	11 10·7
Fri	19	16 36 13 38 14 34 20 23	○ Full Moon Occultation of B.A.C. 4043 (6½) Reappearance of ditto Conjunction of Moon and Jupiter 0° 13′ N. Saturn at quadrature with the Sun	**1st** Oc. R.	7 8 6	11 5·4
Sat	20					11 0·2
Sun	21	7	Jupiter at opposition to the Sun			10 55·0
Mon	22			**1st** Oc. D.	17 45	10 49·8
Tues	23	11 19 11 58 16 18 17 34	Occultation of γ Libræ (4½) Reappearance of ditto Occultation of η Libræ (6) Reappearance of ditto	**1st** Tr. I. **1st** Sh. I.	14 59 15 1	10 44·7
Wed	24		Saturn's Ring: Major axis=41″·65 Minor axis=18″·69	**1st** Oc. D. **2nd** Tr. I. **2nd** Sh. I. **3rd** Oc. D. **1st** Ec. R. **2nd** Tr. E. **2nd** Sh. E. **3rd** Ec. R.	12 11 12 29 12 37 13 47 14 28 37 15 14 15 25 16 53 15	10 39·6
Thur	25	22	Uranus at opposition to the Sun	**1st** Tr. I. **1st** Sh. I. **1st** Tr. E. **1st** Sh. E.	9 25 9 30 11 40 11 46	10 34·5
Fri	26	22 44	☾ Moon's Last Quarter	**1st** Ec. R. **2nd** Ec. R.	8 57 2 10 6 9	10 29·5
Sat	27					10 24·6
Sun	28					10 19·6
Mon	29					10 14·7
Tues	30			**1st** Tr. I.	16 43	10 9·9
Wed	31	8	Conjunction of Moon and Uranus 1° 52′ N.	**1st** Oc. D. **2nd** Tr. I. **2nd** Sh. I. **1st** Ec. R.	13 55 14 42 15 11 16 22 23	10 5·1

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Mar. 1	−45	20 E.	Mar. 11	+12	54 M.	Mar. 21	+71	5 E.
2	57	31	12	0	32	22	58	54
3	69	41	13	−11	28	23	46	42
4	81	52	14	23	39	24	34	32
5	+85	58 M.	15	35	49	25	22	20
6	73	47	16	48	1	26	10	9
7	61	36	17	60	12	27	−2	2
8	49	26	18	72	22	28	14	13
9	37	15	19	84	33	29	26	25
10	25	5	20	+83	16 E.	30	38	36
						31	50	48

Moon farthest from the Earth, Mar. 2, 4h.
,, nearest to ,, ,, 17, 23h.
,, farthest from ,, ,, 29, 21h.

Books received :—Sirius. No. 2. 1886.—L'Astronomie.—The Fortieth Annual Report of the Director of the Harvard College. By Edward A. Pickering. 1886.—The Fixed Idea of the Astronomical Theory. By August Zisbuer. Leipsic : Gustava Fork. 1885.—Bulletin Astronomique de L'Observatoire de Paris. Paris : Gauthier Villars. 1886.—Annuaire de L'Observatoire Royal de Bruxelles. Par F. Folie. 1886. Bruxelles: F. Havey. 1885.—Sidereal Messenger. No. 42.—Astronomische Nachrichten.—Revista de Observatoire. No. 1. Rio de Janiero : Lombasto & Co. 1886.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 280. APRIL. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1886—87.

The first ordinary meeting of the new session was held at the Society's rooms, Burlington House, on the evening of Friday, March the 12th, at 8'clock.

J. W. L. Glaisher, Esq., F.R.S., Fellow and Tutor of Trinity College, Cambridge, the newly elected *President*, occupied the Chair.

The minutes of the previous ordinary meeting were read and confirmed.

Col. Tupman announced that 122 presents had been received during the two months since the last ordinary meeting. None of them called for special notice except a volume of Saunier's Horology, presented to the Society by the translator, Mr. Trippin.

A vote of thanks was passed to the donors.

Col. Tupman read a paper by Mr. Backhouse on the great shower of Andromedes which took place on the 27th November, 1885. Tables were appended giving the number of meteors counted per minute at various periods during the display, the proportion of clear sky to cloud, and other particulars. The maximum of the shower occurred between 6h. 2m. and 6h. 12m. At that time Mr. Backhouse thought the shower was a little richer than that of 1866, and much richer than the shower of 1872. Of a number of meteors, the magnitudes of which were noted, 3 per cent. appeared as big as Jupiter; 11 per cent. were of the first magnitude; 9 per cent. second magnitude, and 20 per cent. third magnitude; 34 per cent. fourth magnitude; 23 per cent. were of the fifth and sixth magnitudes. The bright meteors had trains of a deep red colour; but the faint meteors had no perceptible trains. Those about the third magnitude had trains too faint to show any colour, those of the second magnitude showed red trains; and one which equalled Venus at her brightest, left a train of irregular brightness. As a rule the brighter the meteor the longer the path of duration. Mr. Backhouse had not observed the position of the radiant; it was evident, however, that it was not a point, but an area of several degrees in diameter, in the neighbourhood of γ Andromedæ. Mr. Backhouse drew attention to the fact that an observer in the Transvaal had mapped the paths of several of these meteors, and made the radiant in the constellation Triangulum at more than 10° distant from γ Andromedæ. This was a discordance larger than could be attributed to errors of observation; and he suggested that it may have been due to the paths of the meteors being deflected differently at the two stations, by reason of the earth's attraction. According to Mr. Backhouse's own observations, the larger proportion of the meteors proceeded rather from the side of the radiant area nearest the zenith than from the lower side of the area. A good many meteors were seen on the 28th, 29th, and 30th November, and on the 1st December.

Mr. Ranyard: There is one point I should like to mention with regard to Mr. Backhouse's reference to the position of the radiant observed by Mr. John Ballot, at Rolfontein, in the Transvaal. It is ordinarily assumed that the effects of the earth's attraction may be neglected, and that it makes no sensible difference in the position of the radiant point, because the velocity of meteors in coming up to the earth is usually so great, compared with the velocity they would acquire in falling to the earth during the short time in which they are sufficiently near the earth to be sensibly affected by its attraction, that the original direction of motion is not sensibly affected. Meteors frequently come up to the earth with a velocity of 40 or 50 miles per second, and the time in which they are near to the earth is so small that they acquire very little velocity towards the earth, not enough to sensibly affect their original direction of motion. But in this case the meteors were following up the earth, so that their relative velocity of approach was only about 11 miles per second; therefore, if the effect of the earth's gravity in shifting the radiant can ever be detected, this would be a very favourable opportunity for doing so. A body falling to the earth from an infinite distance acquires a velocity of about seven miles per second. These meteors, if they were moving with a velocity of 11 miles per second, would, neglecting gravity, take about six hours to come from the moon's distance up to the earth, and in that time there would be a very sensible effect produced by the earth's gravity. Meteors following in a path parallel to the plane of the earth's orbit, but below the earth's centre, would be deflected upwards, and those following above the plane of the earth's orbit would be deflected downwards; but whether this will account for the great shift of the radiant referred to by Mr. Backhouse, I do not know. We want some more observations made in the southern hemisphere before it can be said that the theory is confirmed.

The President: I do not think that Mr. Backhouse puts it as strongly as that; he only suggests that the earth's gravity may be the cause of the shift.

Mr. Maunder read a paper on some recently published spectroscopic observations. He said:—There have recently appeared three series of observations of a kind which ought not to take their place as accepted facts without thorough examination. It may seem ungracious to challenge these observations; but I have no idea of calling in question the good faith of the observers, and only draw attention to them with the view of inducing others to make further observations, and give us results which may confirm or discredit those before us. The first observations referred to are a series of observations of bright lines in stellar spectra made by Mr. Sherman, of Yale College. He states that he has seen a number of bright lines in the spectrum of β Lryræ. So far, his observations would not excite scepticism; but he states that he has also examined the spectra of about 50 other stars in which, as yet, no bright lines have ever been seen, and he has detected bright lines in all of them. Here is an important advance in stellar spectroscopy, if these observations may be relied upon; but when we examine the instrumental means used, and which he himself describes, there are indications, I think, of a faulty procedure. Thus, for instance, the slit was not in the focus of the equatorial for the extreme rays of the spectrum. It is difficult to believe that Mr. Sherman's description is that of the behaviour of true stellar bright lines. In the centre he has described them as appearing like stellar points, but at the ends of the spectrum stretching out into broadish lines. Mr. Sherman used an unusually wide slit—about 100 times too wide—that would differ little from no slit at all. Confidence could not be placed in measures with so wide a slit. The suspicious part of the observations is that Mr. Sherman never failed to see bright lines in any star which he examined; and though he says they shone with metallic brilliancy, no other observer has detected them. My own experience is that if dark lines or bands are badly seen, bright lines may appear, because if the real dark lines escape notice, the spaces between may produce the effect of bright lines by contrast with the dark lines. In measures of the motions of stars in the line of sight, I have used a dispersion greater than that of Mr. Sherman, so that my own failure to see bright lines cannot be ascribed to insufficient dispersion.

The second series of observations I wish to refer to, was by Dr. Copeland, of the spectrum of the Nova Orionis. I have no intention of calling in question the observations themselves; the only point is the interpretation to be put upon them. In the spectrum of this star the contrast of light and darkness is unusually pronounced; and it becomes a question whether we should regard the spectrum as a continuous one crossed by dark lines, or as a dull spectrum with vivid bright bands superposed upon it. This is a question similar to that raised by Dr. Draper, in regard to the solar spectrum, and bright lines due to oxygen. Hitherto most observers seem to think they are the former—that is, that the star is an unusually fine specimen of Secchi's third type. Dr. Copeland, however, seems to think the spectrum is not so much a continuous spectrum interrupted by dark lines and dusky bands, as a not very luminous spectrum upon which a series of bright bands are superposed; and he farther identifies some of these bands as those characteristic of hydro-carbon and cometary spectra. Of the importance of that view, if substantiated we cannot doubt. The hypothesis is a very tempting one, but there are several difficulties in that view. I do not think Dr. Copeland has made out his case for a coincidence between the bright spaces and the hydro-carbon bands. The principal hydro-carbon band in comets has a wave length of 5,164 tenth-mètres; but the great dark band in the spectra of the third type has been identified with the b_4 line of the solar spectrum, the wave length of which is 5,167; consequently there is a small but real distance between the edge of the dark band in the star spectrum and the edge of the bright band which Dr. Copeland thinks corresponds to the space just beyond that dark band. He seeks to confirm his theory by the fact that a little way in from the edge of this bright space he sees a bright line, as in the hydro-carbon spectrum we have a second bright line at 5,128; but Dr. Copeland measures the position of his bright line as 5,137. But if we turn to the spectrum of a bright star like Alpha Orionis, Dr. Huggins has found a broad dark line a little way from the b_4 line, the edge of the dark band. This line Dr. Huggins identifies with bismuth, and I think there is little doubt that it is a true metallic line due to bismuth. An additional reason for thinking that these dark bands are really dark bands, and not merely a faint continuous spectrum seen by contrast, is that, in bright stars of the type, if we use a great dispersion we are able sometimes actually to break up these dark bands into individual lines.

The third series of observations to which I would draw attention has been recently published by M. Trouvelot; and deals with the displacements he has observed in the lines of the solar prominences. The observation I specially refer to is the fifth, which he made on June 26, 1874. He found a prominence more than 200 seconds in height, and opened the slit wide enough to take it all in. He closed the slit again to a very narrow slit indeed, and he still saw the prominence absolutely unchanged, at least he so represents it, for he speaks of it in very definite language. And while he was observing it with the slit wide open, it instantaneously disappeared "like a flash of lightning." What he thinks established is that this prominence was receding from him at a tremendous speed, and had gone round the sun's limb and had disappeared. But if we look at his diagram that is not exactly the true inference to draw; the gas must have been moving at every conceivable speed, from rest up to nearly 2,600 km. per second. I think it is sufficient merely to describe these observations to show that there was some mistake somewhere or other: if not, we must give up the view that we have hitherto held, that the displacement of the spectroscopic lines are any true index of the velocity of motion in the line of sight.

The President: Will any member make any remarks on this very interesting paper by Mr. Maunder. The only remark I would make myself is that there was certainly no need for Mr. Maunder's disclaimer with regard to the nature of his criticisms. I am perfectly certain that no critic ever brought forward his criticism in a manner which could be less objectionable to the persons criticised.

Mr. Ranyard: It would be impossible that M. Trouvelot could have seen a prominence with the slit three minutes open. Nobody ever saw a prominence with the slit open more than a very few seconds, the background of continuous spectrum would be too bright. My only theory with regard to M. Trouvelot's observation is, that his slit was radial, and there was a displacement, and a prominence line which M. Trouvelot mistook for a prominence form. If the adapter was loose he might not know the position of the slit.

Mr. Maunder: M. Trouvelot's description is very definite. The diagram shows the prominences with the slit both open and shut.

Mr. Ranyard: He could not have seen the prominence with the slit three minutes open.

Mr. Maunder: If his diagram is to be taken as true to scale, the slit was open more than three minutes in the first place, and was reduced to five seconds in the second observation.

Mr. Horner: I do not think that it is so very extraordinary to see a bright prominence with the slit wide open. I have often seen one myself. I used a Browning Star spectroscope with two very dispersive prisms by Hilger. I do not know how big that slit would be. I have seen them constantly with that on many occasions, and I have seen them with the slit wide open; but you require some care in looking and shading the eye.

Col. Tupman: It is rather a question of the object-glass and focal length.

Mr. Horner: I am in the habit of having the use of a telescope with an aperture of 5-in., and a focal length of 6-feet. With regard to the other point of the bright lines, I should be glad if any Fellow will say whether he has seen bright lines in Gamma Cassiopeiæ, because I have often observed the star, and have never been able to see them with that aperture. I have made all sorts of different arrangements, and have seen Professor Secchi's black lines very often. You can give rise to the most perfect black lines by shifting the prisms—everybody knows that. With regard to the Nova in Andromeda, I could not expect to see its lines with an aperture of that size. I thoroughly endorse what Mr. Maunder says about bright lines. I think it is the easiest thing in the world to mistake the contrast of the black intervals, with the spaces where there are no black intervals, and convert them into bright lines.

The President: Have you seen anything to confirm Mr. Sherman's observations?

Mr. Horner: Nothing whatever; quite the reverse.

Mr. Maunder: My surprise was not that M. Trouvelot saw the prominence with the slit open, but that he saw it with the slit shut. (Laughter.) I do not know that I mentioned that the slit he used was a tangential slit, and not a radial slit.

Mr. Horner: I am in the habit of observing with both. It is a most interesting thing to see the real height of a prominence, which you cannot always see unless it is bright. With an exceedingly bright prominence you can see it easily with the whole slit open. Mr. Hickley, who is well known, and has contributed papers on the prominences, will certainly confirm this, because I have often observed with his three compound prisms arrangement by Browning.

The President: If no other Fellow desires to make any further remarks, I am sure you will give Mr. Maunder a vote of thanks. His paper may cause Mr. Sherman to explain more fully his method of observation.

Mr. Knobel read a paper by Professor Pritchard on the new star in Orion, photometrically, and spectroscopically observed at the Oxford University Observatory. Between December 22nd and January 10th, a diminution of magnitude was observed from magnitude 6.46 to magnitude 7.09. The brightest colour of the spectrum on December 23rd was a red orange, on December 29th two fresh bands were seen in the red. As to the character of the spectrum it was seen at Oxford with six absorption bands. The apparently contradictory results obtained by different observers of the spectrum of this star are very perplexing.

Mr. Maunder: I would only refer to the last sentence. I do not think there is any real contradiction in the actual observations made at Dunecht, and at Greenwich, as to the spectrum of the Nova Orionis, but simply as to the interpretation to be placed upon what has been seen. We have described a spectrum in which dark and bright spaces follow each other alternately. As to the position of the limits of those bright and dark spaces, I think there is very little doubt that all the observations that have been made hitherto are practically in very good accord. The only question is as to whether we are to regard the bright spaces as being the feature of the spectrum, and as really proving the existence of bright bands, or whether we are to regard the dark spaces as being the feature of the spectrum, and as being true absorption bands. I think that this star may lead us to the conclusion that stars of this type, which are variable, vary mostly on account of the change in brilliancy of the continuous spectrum, and not, as has been supposed, in the intensity of the absorption bands. I think it is clear in this particular instance that it is an increase of brilliancy in the continuous spectrum which has given rise to the greater present light of the star.

A vote of thanks was passed to Professor Pritchard on his paper.

Mr. Chambers said: I should like Mr. President to mention to the Society that I have been engaged for some years past in drawing up a catalogue of red stars for general use. I hope to be able to complete the catalogue and publish it some time during the coming summer; and I should be very glad to receive from any Fellows of the Society, who have picked up isolated red stars which have never been catalogued, notes with regard to any such stars. I have already received several additions to my list, but I think it is likely many other Fellows of the Society may have noted chance stars, and if they will forward particulars to me I shall be very glad to receive them.

The following papers were presented, and taken as read :—

J. Tatlock: *Occultations observed at Beloit, Winconsin, U.S.A.*

J. E. Gore : *On the orbit of* 40 (o^2) *Eridani.*

Rev. T. E. Espin : *Some new red and orange red stars.*

J. Tebbutt : *Observations of the conjunction of Saturn and* μ *Geminorum.*

E. B. Powell : *Additional remarks on the periodic time of* α *Centauri.*

Rev. S. J. Perry : *Occultations of stars by the Moon, and phenomena of Jupiter's satellites observed at Stonyhurst in the year* 1885.

Communicated by the Astronomer Royal : *Observations of Comets d* 1885 (*Fabry*), *and e* 1885 (*Barnard*), *made at the Royal Observatory, Greenwich.*

J. G. Lohse : *Observations of the new star in Andromeda, made at Mr. Wigglesworth's Observatory with the* 15·5 *in. Cook Refractor.*

The meeting adjourned at half-past nine o'clock.

SELENOGRAPHICAL NOTES, APRIL, 1886.

By Thos. Gwyn Elger, F.R.A.S.

The Sabine and Ritter Cleft system. The strange discrepancies between the published drawings and descriptions of the closely associated clefts crossing the smooth grey plain north-east of Ritter and the region east of Arago and Maclear, may possibly be due to the extreme tenuity and delicacy of some of these objects, and the consequent difficulty of tracing them, save under exceptionally favourable conditions; but when we come to study the positive and negative results of Mädler's observations, and to compare the drawings of Schmidt with those of other observers, the probability of the existence of some different, or rather additional cause to account for this want of agreement may be fairly admitted. The accurate and keen-sighted Gruithuisen appears to have been the first to detect anything in the form of a cleft on the grey surface between Ariadæus and Ritter. He described it as an apparent prolongation of the great Ariadæus cleft to the east of Ritter, and towards the well-known clefts (discovered by Lohrmann, in 1823) running in a westerly direction from the south of Sabine. Mädler searched in vain for this cleft on several occasions with the great Berlin refractor, and only after many attempts succeeded in detecting the northern end of it, extending from the west side of Ariadæus to a neighbouring crater south-west of the latter formation, while Schmidt, in 1851, traced it from Ariadæus to Ritter C, a conspicuous crater north of Ritter. He shows, however, a notable break in its course at a point rather less than half way between its two extremities. The section north of this gap is, as he shows it, somewhat coarse and irregular in width, and answers to the portion seen by Mädler. Dr. Klein describes it (*Sirius*, July, 1882) as running east of Mädler's crater, an observation which I have frequently been able to confirm. The following notes were made on 11th March, 6h. 30m. to 8h. 0m., when I had a fairly satisfactory view of the clefts in this region, with a power of 340 on my 8½-in. Calver, the east wall of Julius Cæsar being on the morning terminator:—1. A cleft from Ariadæus running towards Ritter C, passing on the east side of a prominent crater a little south-west of Ariadæus, and terminating a few miles south of this crater. It is an apparent prolongation of the great Ariadæus cleft. 2. A very delicate cleft from near the south end of the last towards Ritter, but not traced up to the latter formation, owing to the intervention of the broad shadow of Ritter C. These two clefts correspond to No. 22 in Schmidt's *Rillen*, and the northern section of 1 to the cleft seen by Mädler (No. 24 in his catalogue). 3. A cleft from a small bay or indentation in the mountain border extending from Dionysius to Ariadæus, about midway between the two, to the east side of Ritter. The southern portion of it, after passing very close to the last cleft, curves rather sharply up to the border of Ritter. Schmidt describes this cleft, which is numbered 24 in his *Rillen*, as being connected with the system west of Sabine. No such connection, however, is shown in his chart, where he draws it as I see it. It is probably an extension of the branch η (*see Neison*, *Map II.*) of the great Ariadæus cleft. Although this latter object has not been actually traced to the notch which marks the northern end of the cleft, it has been followed by Gaudibert very nearly up to the rocky border stretching between Ariadæus and Dionysius. The night of March 11th was at times particularly favourable for the examination of this district. The southern end of the great Ariadæus cleft, with its wide sloping sides and uneven border, was beautifully distinct, and the branch η, almost as wide and deep at its junction with the main cleft, resembled a railway cutting seen from above, gradually diminishing in depth and width as the line emerges from the hill it traverses. At times of best definition I could see a thread-like prolongation of η to within four or five miles of the north end of the cleft from Ritter, but not actually up to the rocky border, though from the trend of the two clefts one felt sure that with greater power and more perfect definition the connection would be revealed. The direction of this cleft from Ritter is prolonged in the region lying south of the great Ariadæus cleft by a row of little mounds or hills, running nearly parallel to the latter. This would hardly deserve noting were not these prolongations so commonly observed in connection with other clefts, that the association cannot be due to chance, but must be occasioned by some physical cause which is not, in the present state of our knowledge, easy to explain. 4. A cleft from the eastern wall of Ritter to the west side of Dionysius: seen as a delicate, dark, straight line, without any appreciable light border. Later on I detected the remarkable crater-row, which, commencing at the northern end of this cleft, curves round the outer slope of Dionysius; but I failed to see the further extension of it, which, according to Schmidt, joins the northern end of 3. 5. A cleft from near the mountain δ, north-west of Ariadæus, to the east of a crater east of Manners; very difficult, and only seen by glimpses. Gaudibert has traced it from Sosigenes *a* to the west side of Sabine; but Schmidt shows a break in it east of Manners, drawing the southern section as running from the crater just mentioned to a crater north of Sabine, and the southern section as I saw it on March 11th. 6. A coarse and easily seen short cleft from two craterlets north of Sosigenes *a*, by the west side of *a*, to a crater near the mountain δ. 7. A short, delicate cleft, crossing the last a little north of δ (29 in Schmidt's *Rillen*). 10, 11. Two closely-parallel delicate clefts, between Maclear and Sosigenes, trending towards Sosigenes *a*, the more westerly of which appeared to be a northern extension of 6. 8. A very obvious cleft from the craterlets north of Sosigenes *a* to the eastern wall of Arago. 9. An extremely delicate cleft running from the same craterlets towards a large low mound north of Arago. These objects (26 and 27 in Schmidt's *Rillen*, and 20 and 21 in Mädler's Catalogue) were discovered by Mädler in Oct., 1832; but, strange to say, could never be found by the former observer, and were only seen once by the latter. Gaudibert saw both in part in 1874 (see *English Mech.*, vol. xviii., p. 639), and described them as difficult. He has subsequently traced 8 to Arago. On March 11th I found 8 to be by far the easiest cleft in the neighbourhood, a circumstance which renders Schmidt's many fruitless attempts to see it inexplicable by any supposition relating merely to the position or illumination of the object.

Galileo's rank in the Moon. From Mr. Allen's letter in last month's *Register*, it appears that I misapprehended the nature of his proposal, which, when the much-needed revision of lunar nomenclature is taken in hand, might well be duly considered.

Kempston, Beds: 19 March, 1886.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE RADIANT POINT OF THE ANDROMEDES of November 27, 1885—Andromedes and Leonids in 1965–66.

I have collected in the following list a number of positions given by some of the best observers for the radiant point of this brilliant display. The table might be considerably extended if many other less definite and satisfactory determinations were included, but I think these would only introduce elements of error. The results here compared will suffice to give a good mean position for the centre of radiation :—

Observer and Station.	*Radiant Point.*		*Reference.*
	α	*δ*	
	°	°	
Copeland, Dr. R., Dun Echt.	25·9	+46·4	*Month. Not.*, Dec., 1885, p. 67.
Corder, H., Chelmsford ...	25	+44	Private communication.
Denning, W. F., Bristol	24 23	+43·5 +45	*Month. Not.*, Dec., 1885, p. 81.
Denza, P. F., Moncalieri :			
7h. 35m.	22	+44	*Nature*, Dec. 17, 1885, p. 151.
8 20	26	+43	*Month. Not.*, Dec., 1885, p. 78.
9 8	28	+42	*Observatory*, Jan., 1886, p. 67.
Gautier, E., Geneva ...	23	+43	The *Times*, Nov. 30, 1885.
Genieis, Mons., St. Pons, France	25	+45	*L'Astronomie*, Jan., 1886, p. 29.
Grant, Prof. R., Glasgow ...	24	+45·5	*Month. Not.*, Dec., 1885, p. 64.
Hildebranson, H., Upsala...	23	+45	*L'Astronomie*, Jan., 1886, p. 34.
Jenkins, C. A., Oxford ...	23	+44	*Month. Not.*, Dec., 1885, p. 66.
Kamp, T. W., London ...	25	+46	*English Mechanic*, Dec. 1885.
Nach, W. C., Greenwich ...	20	+49	*Month. Not.*, Dec, 1885, p. 63.
Perrotin, Mons., Nice ...	24	+43	*L'Astronomie*, Jan., 1886, p. 33.
Plummer, W. E., Oxford...	21	+45	*Month. Not.*, Dec., 1885, p. 66.
Rayet, Mons., Bordeaux ...	28·5	+46	*L'Astronomie*, Jan., 1886, p. 35.
Ricco, A., Palermo ...	25	+41	*L'Astronomie*, Jan., 1886, p. 33.
Robinson, W. H., Oxford	22·5	+42	*Month. Not.*, Jan., 1886, p. 120.
Sawyer, E. F., Cambridgeport, U.S.A.	22·5	+42·5	*Observatory*, Feb., 1886, p. 96.
Schiaparelli, G. V., Milan :			
6h. 35m.	15	+45	*Nature*, Dec. 17, 1885, p. 151.
7 12	18·5	+44	*Month. Not.*, Dec., 1885, p. 79.
8 7	23	+42	*Observatory*, Jan., 1886, p. 67.

Observer and Station.	*Radiant Point.*		*Reference.*
	α	*δ*	
	°	°	
Scoles, Rev. J., Malta ...	21	+44	*Month. Not.*, Dec., 1885, p. 83.
Strahan, Major G., Agra ...	26¼	+46	*Month. Not.*, Jan., 1885, p. 122.
Smieton, J., Dundee ...	21	+46	*Nature*, Dec. 3, 1885, p. 102.
Stephen, Coggia, & Borelli, Marseilles	22·5	+49	*L'Astronomie*, Jan., 1886, p. 35.
Tramblay, Mons., Orange, France	30·5	+41	*L'Astronomie*, Jan., 1886, p. 29.
Tupman, Lieut.-Colonel G. L., Harrow	27	+44	*Month. Not.*, Dec., 1885, p. 80.
	22·5	+45·5	
Ward, Col. M. F., Partenkirchen, Bavaria ...	23	+43	*Month. Not.*, Dec., 1885, p. 84.
West, R. H., Beyrout, Syria.	25·5	+43·5	*Nature*, Dec. 17, 1885, p. 152.
Young, Prof. C. A., Princeton, N.J., U.S.A. ...	27·5	+43·5	*Sidereal Messenger*, Jan., 1886, p. 23.

The mean of the 33 positions is at $23^{\circ}{\cdot}7+44^{\circ}{\cdot}3$. The computed radiant point of meteors belonging to Biela's comet is $23^{\circ}{\cdot}5+43^{\circ}{\cdot}0$ from the orbit of 1852, corrected for precession (*Monthly Notices*, Dec., 1885, p. 80). Of the great shower of November 27, 1872, Professor A. S. Herschel, who collected a considerable number of results, says: "The principal region [of radiation] comprises a compact group of about 35 observations, having their centre, or average place, at $25^{\circ}{\cdot}1+42^{\circ}{\cdot}9$." The three places are in very fair agreement. The latter is possibly a little south-east of the true place, owing to the inclusion of several estimates, placing the radiant approximately at the star γ Andromedæ. It is obvious that in cases of multiple observations of a meteor shower, from near a bright star, some of the less accurate observers will find it sufficient to assign the radiant exactly at the star's place, whereas the point may really, as in the case of these Andromedes, be situated several degrees distant. In the preceding table I have endeavoured to ensure accuracy by omitting the positions thus assigned, and selecting such values as are definitely expressed in degrees of R.A. and Declination.

I would here make a brief reference to an interesting eventuality. Biela's comet, and its allied meteor shower, participate in a rapid retrograde displacement of the node. This, of course, brings the shower earlier at each return. The computed date of the earth's passage through the node was: December 10, for 1772; December 4, for 1826; and November 28, for 1852 (*Monthly Notices*, Vol. XXXII., p. 357). This nearly corresponds with the apparition of the Andromedes observed by Brandes, on December 7, 1798, and recently by other observers on November 27, 1872, and 1885. Now, in the case of the other great November shower—the Leonids—there is a gradual increase in the longitude of the node, amounting to some 29′ during one revolution (33·25 years) of the swarm. Thus, the display occurs later at each return, though the change is very slight, amounting to about 1½ days in a century.

These variations in the Andromedes and Leonids, acting in contrary directions, will ultimately, by their cumulative effect, bring the two showers coincident as regards date of apparition. In fact, future observers may possibly witness a meteoric spectacle, certainly without parallel, in about 1965–6. The Leonids should return on November 16–17 of that year, and the Andromedes—if the node retrogrades at the same rate as observed between 1798 and 1872–85—will be due at that epoch; but it is difficult to say whether the most condensed part of the stream will intersect the earth that year, though it seems a very possible deduction. Between the displays observed in 1798 and 1885, there is an interval of 87 years, equal to 13 periods of 6·7 years. This period corresponds well with the display of 1838; 12 similar periods, counting from 1885, brings us to 1965–6, which nearly agrees with the apparition of the Leonids, which will brilliantly occur at that epoch. Moreover, the Andromedes are probably distributed in high condensation along a considerable arc of the orbit, so that, though the period may hereafter be found not exactly conformable with the return of the major group in 1965 or 1966, there may be still a rich display of them, especially should the meteor cluster go on lengthening out, in the interim. And it must be remembered that the Leonids appear in considerable strength during several successive years near the maximum, so that there is almost certain to be a contemporaneous display of the two great November showers near the period indicated.

The phenomenon will doubtless be anticipated by a future generation of observers. Should it happen in its most imposing aspect, and under mutually propitious circumstances, it will be as magnificent as it is unique. The very swift and brilliant Leonids, with their phosphorescent streaks, radiating from the eastern sky, will offer a strong contrast with the very slow, gradual flights of the trained Andromedes, proceeding from a great altitude in the western sky. Indeed, the dissimilarity of aspect and diversity of motion in the meteors will be most remarkable, apart from the mere effect of their great numerical strength, which the combined display will naturally exhibit.

In conclusion, I may mention that Sir R. S. Ball, in his new and admirable work "The Story of the Heavens," mentions that during the display of November 27, 1872, he counted 300 meteors per hour. This will convey an inferior impression of that shower, for there were certainly more than 3,000 meteors per hour to be seen by one observer at places where the sky was clear.

W. F. DENNING.

Bristol: February 12, 1886.

REVIEWS.

Spectrum analysis in its application to terrestrial substances, and the physical constitution of the heavenly bodies. Familiarly explained by the late Dr. H. Schellen, Director der Realschule, I. O. Cologne, &c., &c., &c., associate of several learned societies. Translated from the third enlarged and revised German edition by Jane and Caroline Lassell. Edited, with notes, by Captain W. de W. Abney, R.E., F.R.S. With numerous wood-cuts, coloured plates, and Angström's and Cornu's maps. Second edition. Longmans, Green & Co., London. 1885. pp. xxiv. 626. Price 31s. 6d.

Dr. Schellen did not live to complete the reconstruction of his excellent work, which was done by his friend, Dr. H. J. Klein. It is superfluous now to praise the translators. We may assume that we have a faithful rendering of the German, and we are sure that it is in clear, good English, and reads more like an original than a version. English readers are greatly indebted to the Misses Lassell for affording them access to what may be considered as a standard work on the beautiful and refined branch of science to which it relates. The editorship of Captain Abney has materially added to its value. He has revised and somewhat remodelled the whole, enriching it with notes which either modify, correct, or enlarge upon various parts of the text, with which the editorial notes by Dr. Huggins to the former English edition have now been incorporated by Dr. Schellen.

Part I. is on the artificial sources of high degrees of heat and light. II. Spectrum analysis in its application to terrestrial substances. III. Spectrum analysis in its application to the sun. IV. Stellar spectroscopes and investigation of the moon and planets by spectrum analysis. V. Application of spectrum analysis to the fixed stars. VI. Results of the spectroscopic investigation of nebulæ and clusters. VII. Results of the spectroscopic investigation of comets and meteors. VIII. Results of the spectroscopic investigation of the zodiacal light, aurora borealis, and lightning. There are eleven appendices. The plates, fourteen in number, comprise a photograph of the nebula in Orion, by Mr. Common (frontispiece). Total solar eclipses and corona (two tinted), solar spectrum and spectrum of the prominences, solar prominences observed by Zöllner and Young (coloured), Angström's maps of the solar spectrum (four plates, which would extend thirteen feet in a line), Cornu's map of the ultra-violet portion of the solar spectrum (this is from H to U, and may be regarded as a direct continuation of Angström's spectrum. It would be nearly four feet in a line); Huggins' photographic spectra of the fixed stars (seven in number; α Lyræ, Sirius, η Ursæ Majoris, α Virginis, α Aquilæ, α Cygni, Arcturus); and emission and absorption spectra in colours (containing 10 spectra).

There are 291 illustrations in the text, one coloured, of the diffraction and refraction spectrum. Without the appendices, there are 62 tables of elements, lines, indices of refraction, wave-lengths, &c. Appendix G is a long table of the measurements of the wave-lengths of the Fraunhofer lines, by A. J. Angström. Appendix L contains Lockyer's tables of the coincidence of the Fraunhofer lines with the lines of metals; and, finally, there is an index. It is altogether a fine volume.

We extract the following from pp. 450, &c., about the results of Huggins' labours on the photographed spectra of stars. The first six spectra of the stars above mentioned are those of white stars, belonging principally to one type of spectrum, consisting of twelve strongly-marked lines, ill-defined at the edges, four of which are most probably due to hydrogen. In the spectrum of Vega, besides the 12 lines, Huggins suspects a very fine line between H and K. The continuous spectrum extends as far as S in the ultra-violet. The spectrum of Sirius is very similar to that of Vega, as also that of η Ursæ Majoris. In the spectrum of α Virginis the twelve typical lines have already begun to diminish in width, and become defined at the edges; a great number of very fine dark lines have also made their appearance. Besides this, in the spectrum of α Aquilæ numerous other lines are also present, so that the general appearance of the spectrum has begun to approach to the solar type, to which the spectrum of α Cygni comes still nearer. The spectrum of Arcturus differs somewhat from the foregoing, and belongs to the class in which the sun is placed. The whole photographic spectrum appears to be traversed by lines similar to those in the solar spectrum; 21 of the principal lines have been measured; some of them are coincident with the solar lines. In the spectrum of Rigel, all the typical lines are visible; they are brighter than in the spectrum of α Cygni, but not so bright as in the spectrum of α Virginis. The spectrum of the star Belelgeux could only be obtained with difficulty, as after an exposure four times the length of time needed for Sirius, a photograph was produced, yielding a very limited spectrum, the most distinct portion being in the neighbourhood cf G. The spectrum of Aldebaran also required a very long exposure. The spectrum of Capella extends in the photograph from F to S, and resembles so closely the solar spectrum that the one could be taken for the other. Huggins concludes that this star is in a state of development very similar to that of our sun. Henry Draper divides the spectra into two classes; those that have a resemblance to the solar spectrum, and those in which the lines are comparatively few, but of greater breadth and intensity. He concurs with Huggins in regarding the spectra of Arcturus and Capella as almost identical with that of the sun, whereas the spectra of Vega and α Aquilæ are totally different. The photographs taken by Draper have not yet been published.

A few errata are marked, and we note also the following:—P. 427, line 10 from bottom, "lower" should be *upper*; in the next line, "upper" should be *lower*; in the second line from the bottom, "beneath" should be *above*. Page 442, "Argo" (twice), should be *Argûs*. Page 534, lines 15 and 16, "lengths" should be *longitude*. Page 549, line six from the bottom, "below" should be *above*.

In the February number of *L'Astronomie* is a letter by MM. Paul and Prosper Henry on their recent discovery of the spiral nebula in the Pleiades, which touches the star Maia. They give a drawing of the nebula. "Photography at the Paris Observatory" is a valuable article by M. Flammarion, with illustrations. 1. The double equatorial, with clockwork employed for photography. 2. Wolf's chart of the Pleiades in 1874. 3. Photograph of the Pleiades by MM. Henry, comprising over

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1,000 stars. 4. Chart of the Pleiades, showing the Merope nebula by Tempel in 1860. 5. Reproduction of a photograph of a region in Cygnus, about 4° square. 6. Photograph of Saturn. The same number contains a paper, illustrated, about the Aurora Borealis, after M. A. Angot.

Report of the Superintendent of the United States Naval Observatory, for the year ending June 30, 1885. Washington: 1885. pp. 14.

Urgent need is felt for the erection of a new observatory, the site of which was purchased in 1881. Commodore Geo. E. Belknap, the present superintendent, suggests that, in order to keep the continual services of the trained assistants, they should be regarded as permanent attachés of the Institution, with adequate renumeration for their services. The appointment of a Board of Visitors, who should annually visit the Institution, is again recommended.

The annular eclipse of March 16, 1885, presented no unusual phenomena. There is some prospect of an observing party being sent to the west coast of Africa, near Benguela, to observe the total eclipse of August 29, 1886. The duration of totality is 4m. 40s. there.

Records show that only about one night in eight is good for observing, and that an exceptionally good night cannot be reckoned upon much oftener than once a month.

The organization of the Institution, and the methods pursued, being in as good working condition as the present means and force admit, no change has been made.

It is much to be hoped that Congress will soon determine on the erection of the new observatory, as fully three years would be required to get all in working order. The present location is very unhealthy, and unsuited for astronomical work, whereas the surroundings and atmospheric conditions at the new site are very favourable.

British Rainfall, 1884. On the distribution of rain over the British Isles during the year 1884, as observed at more than 2,000 stations in Great Britain and Ireland, with articles upon various branches of rainfall work. Compiled by G. J. Symons, F.R.S., Secretary of the Royal Meteorological Society, &c. London: Edward Stanford, Charing Cross. 1885. pp. 242. Price 10s.

Mr. Symons says:—"If, in 1861, any one had predicted that twenty-five years afterwards the preparation of the report on the rainfall of a single year would (besides extra work for others) occupy the whole time of three persons for six months, one of those persons not unfrequently working at it from 9 to 12 hours per diem, I should have considered the prediction wild in the extreme. But it would have been true; and a high standard of accuracy cannot be maintained without an amount of examination and verification which few persons would credit; I should, however, be glad if it were rendered possible to secure equal efficiency with rather less strain upon myself." The observers' accounts of the small rainfall of the year occupies 127 pages, to which the author himself has added a somewhat full account of the drought. He says: "I think it will be freely admitted that, whether it be for the drought, for the thunderstorms, or for the great Norfolk rains of September 3rd and 4th, the record of 1884 is well worth all the labour which has been bestowed in its compilation." Notwithstanding the district above alluded to is a very dry one (perhaps a mean fall of 24-in. in a year), 16 stations had 3-in. or upwards in two days, and two stations upwards of

4-in. At Seathwaite, in the English Lake District, 6·78-in. fell on May 8th, which is the heaviest fall recorded in the British Isles at any station on any day for 20 years. This volume has the monthly maps of rainfall, and a map showing the difference of total rainfall in 1884 from the average 1870—79.

Telegraphic Determinations of the difference of Longitude between Karachi, Avanashi, Roorkee, Pondicherry, Colombo, Jaffna, Muddapur, and Singapore, and the Government Observatory, Madras. By Norman R. Pogson, C.I.E., F.R.A.S., and F.M.U., Government Astronomer at Madras. Madras: 1884.

To the supineness of the Madras Government in years past it seems owing that we have not heard much of the work done in this Observatory. This publication is very creditable to Mr. Pogson, who is still obliged to lament the want of a good cylinder chronograph. The first approximate determination of the longitude of the Madras Observatory was by Mr. John Goldingham, who used eclipses of Jupiter's satellites; the observations, however concordant, gave a value about ten seconds too great. His successor, Mr. T. G. Taylor, obtained a value much nearer the truth by moon culminations. The present adopted longitude, 5h. 20m. 59·4s. rests upon the observations of the members of the Transit of Venus Expedition in 1874, combined with others in the great Trigonometrical Survey of India, in 1876—77, and is of great exactness.

Mr. Pogson states certain drawbacks attendant on the determination of some of the above-mentioned longitudes, such as the unknown personal equation of some concerned; but he has, nevertheless, made the very best of his circumstances, as one or two specimens will show: the trigonometrical difference of longitude between Madras and Pondicherry (1880) is only 0·03s. less than was found by MM. Fleuriais and Pogson in 1870. The longitude of Roorkee, deduced trigonometrically, is 0·43s. greater than the telegraphic determination; this is, however, accounted for by the geodesical position of that place. We find, from the *Madras Weekly Mail*, September 12, 1885, that a volume of magnetic observations made during the years 1851—55, has also been issued from the Madras Observatory. "The removal of all unnecessary restrictions by the present Governor, will ensure," Mr. Pogson says, "the earliest possible publication of all past results on hand, and will give renewed energy to the future exertions of the oldest established English Colonial Observatory. This will be good news to many who know how much valuable astronomical work has been done in Madras during the last twenty years, and who were beginning to fear that it was likely to remain buried in the archives of the Observatory." Every facility was given by the Telegraph Department in the determination of the longitudes.

Newton, his Friend, and his Niece. By the late Augustus de Morgan, Edited by his wife, and by his pupil, Arthur Cowper Ranyard. London: Elliot Stock, 62, Paternoster Row, E.C. 1885. pp. vi., 161. Price 5s. 6d.

For nearly a century and a half there has existed a smothered rumour, derived from contemporary assertions, which it has been customary to keep out of history, that the niece of the great Isaac Newton, the beautiful and accomplished Catherine Barton, the "famous witty Miss Barton," was the mistress of Charles Montague, Earl of Halifax, Newton's old and close friend, and at last his political patron. There are neither frequent nor decided allusions to this rumour in biographies of Newton. Some very difficult recorded circumstances give it a strong air of truth, and might have led to a suspicion that Newton had connived, it being known that his connection with Halifax suffered no interruption.

De Morgan gives the grounds which satisfy him that there was a private marriage between Newton's old friend and his favourite niece, contracted in 1706, known and acknowledged in their private circle, but not published to the world. In her preface Mrs. De Morgan says: "It is needless to say that the justification of Newton in the matter of his niece's marriage was not, in the writer's estimation, merely a subject for literary or historical gossip. His intense reverence for Newton would have given way before the clear conviction of a grave defect in moral character. He had been compelled to admit the culpable weakness, to call it by the mildest name, of Newton's conduct to Flamsteed; but a continued countenancing of immorality, by which he was supposed to be himself the gainer, would have been, in my husband's view, enough to darken even Newton's intellectual brightness. He believed, however, that the suspicion of this had been cleared away, and the arguments which led to this belief are placed before the reader. The question is not an unimportant one to those who hold moral excellence to be of more value than intellectual power, and who are sometimes apt to believe the possession of the one implies that of the other."

No one could have produced a vindication of Newton in the matter in question better than the late regretted Augustus de Morgan. He was in every way admirably qualified for the work; and we think that the conclusion at which he arrived will find general acceptance. We owe thanks to the editors for this book, which also, apart from its main object, contains much entertaining reading.

All Past Time. Classification of all the eclipses from Creation, and all the transits of Mercury and Venus, &c. By J. B. Dimbleby. London: The Book Society, 28, Paternoster Row, E.C. Price 6d.

The above is only a small part of the title of this pamphlet of over 100 pages. On the cover in pencil the words "*For opinion*, not review," relieve us from the task of criticising what must have cost much, though we fear misspent, labour on the part of the author, and the "British Chronological and Astronomical Association."

Annals of the Astronomical Observatory of Harvard College, Vol. xiv. Part I. *Observations with the Meridian Photometer during the years* 1879-82. By Edward C. Pickering, Director, aided by Arthur Searle and Oliver C. Wendell, assistants in the Observatory. Cambridge: John Wilson and Son, University Press, 1884. Pp. viii. and 324.

The observations in this volume were designed for obtaining a satisfactory determination of the comparative brightness of all stars not fainter than the sixth magnitude, which could be conveniently observed in the latitude of the observatory. They were begun on October 25th, 1879, and completed on September 17th, 1882. The number of stars included in the catalogue is 4,260. The observations comprised 700 series, and included 94,476 separate comparisons. Adding to these the preliminary observations and observations of objects not included in the catalogue, the total number of comparisons will exceed one hundred thousand. The instrument employed is known as the broken transit, in which the line of sight is always horizontal, and stars of all altitudes may be observed without moving the head or eyes. All the stars having been observed near the meridian, afforded the advantage of their being easily found, and of their being then at their greatest altitude.

Chapter I. is a description of the Meridian Photometer, with plates, and the method of observation and reduction. The standard of comparison is the Pole-star. "Two persons are required to measure the light of a star with this instrument; the observer, with his eye at the eye-piece, brings the image of the Pole-star into the middle of the field by the two handles moving the northern prism; the recorder directs the southern prism to the star to be measured by means of the declination circle; a time-piece before him enables him to select the proper stars for successive observation. By the collimating screw the star may be brought to the centre of the field. The observer places the image of the Pole-star near that of the star to be measured, and makes a setting by turning the Nicol until the images appear to be equal. He then notifies the recorder to enter the reading, which is done to tenths of a degree. On being notified by the recorder that the reading has been made, he turns the nicol so that the image of the Pole-star just disappears, and then regains a brightness equal to that of the other star. This second setting is recorded while the observer reverses the images by moving the northern prism. Two more settings are then made in the same way as before."

Chapter II. contains the Summary of Series, and amongst other tables, one giving results of observations for variation of the Pole-star. Chapter III. is on Atmospheric Absorption. Chapter IV. Eye Estimates. V. The General Catalogue, and VI. Miscellaneous. Stars, not more than 30° south of the equator were taken from nine catalogues or lists, as well as from the chief national ephemerides for 1880, the epoch of the catalogue.

"Important systematic variations have been shown by the present work to exist in many catalogues. These variations depend upon the part of the sky observed, and especially upon the neighbourhood of the Milky Way, where the preponderance of bright stars sensibly diminishes the apparent brightness of the adjacent fainter stars. The scale of magnitudes in general use consequently varies as much as three or four tenths of a magnitude in different parts of the sky. These variations will be fully discussed in Part II. of the present work, where also will be found a discussion of those stars which may be suspected of variability. . . The additional information to be supplied in Part II. will often be necessary to enable real cases of variability to be distinguished from accidental variations in the magnitudes assigned to the same star at different times."

The ancient observations by Ptolemy, Sûssi and Ulugh Beg, are also referred to. The discussion of these will appear in Part II. A study of the introductory chapters, with XXVI. tables, and many formula, which occupy 100 pages, shows that no pains have been spared nor any precautions neglected to render this catalogue as nearly perfect as possible. The work is one to form an era in the department of the science to which it relates; and Professor Pickering and his able assistants deserve the thanks of all astronomers for so much labour, well planned and skilfully executed.

The Reaction of Gravity in Motion; or the Third Motion of the Earth By Trevigra. London: The London Literary Society, 376, Strand, W.C. 1s.

An anti-Newtonian pamphlet. We need say no more. Trevigra seems to be one of Archbishop Whately's *puzzle-headed* men.

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator. E., Evening Terminator.

1886.	° ′		° ′		° ′
April 1	—62 59 E.	April 11	— 4 54 M.	April 21	+53 6 E.
2	75 10	12	17 6	22	40 53
3	87 22	13	29 19	23	28 41
4	+80 26 M.	14	41 30	24	16 29
5	68 15	15	53 42	25	4 17
6	56 3	16	65 54	26	— 7 56
7	43 52	17	78 5	27	20 8
8	31 40	18	+89 41 E.	28	32 21
9	19 29	19	77 30	29	44 32
10	7 17	20	65 17	30	56 45

Moon nearest the Earth, April 14, 18h.
,, farthest from ,, ,, 26, 16h.

THE PLANETS FOR APRIL.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	1 32 30	N.12 15	10″·0	0 43·3
	9th	1 4 11	N. 9 2½	11″·6	23 49·7
	17th	0 49 29	N. 5 11	11″·2	23 3·5
	25th	0 51 28	N. 3 25	9″·8	22 34·1
Venus ...	1st	22 4 58	S. 8 30	34″·8	21 22·4
	9th	22 28 22	S. 7 35	31″·0	21 14·3
	17th	22 54 45	S. 6 5	27″·6	21 9·1
	25th	23 23 10	S. 4 4½	25″·0	21 6·0
Mars ...	1st	10 41 4	N.11 51	15″·2	10 0·3
	9th	10 36 16	N.11 57½	14″·4	9 24·1
	17th	10 34 36	N.11 45	13″·6	8 51·0
	25th	10 35 54	N.11 16	12″·8	8 20·8
Jupiter ...	1st	12 1 18	N. 1 35	41″·2	11 20·4
	9th	11 57 48	N. 1 57	41″·0	10 45·4
	17th	11 54 40	N. 2 16½	40″·4	10 10·9
	25th	11 51 2	N. 2 32	39″·8	9 36·8
Saturn ...	1st	6 9 43	N.22 50	16″·4	5 29·7
	9th	6 11 46	N.22 50½	16″·0	5 0·3
	17th	6 14 15	N.22 51	15″·8	4 31·4
	25th	6 17 6	N.22 51	15″·6	4 2·7
Uranus ...	3rd	12 20 43	S. 1 25	4″·0	11 31·9
	19th	12 18 20	S. 1 10	4″·0	10 26·6

Mercury sets about an hour and a quarter after the sun at the beginning of the month, the interval rapidly decreasing. Afterwards he rises a few minutes before the sun, the interval increasing to about half an hour.

Venus rises about an hour and a half before the sun throughout the month.

Mars sets about half an hour before sunrise at the beginning of the month, the interval increasing.

Jupiter sets on the 1st just before sunrise, the interval increasing.

Saturn sets an hour and three quarters after midnight at the beginning of the month, the interval decreasing.

DUN ECHT CIRCULAR, *No.* 113.

Ephemeris of Comet "Barnard" computed from Professor Krüger's elements in *Astronomische Nachrichten*, No. 2706.

Ephemeris for Berlin Midnight.

1886.	R. A.			Decl.			log. Δ	log. *r*
	h.	m.	s.		°	′		
March 31	1	50	22	N.	30	45·4	0·2006	9·9509
April 4	1	49	33		32	15·9	0·1868	
8	1	48	25		33	49·1	0·1699	9·8776
12	1	46	58		35	23·8	0·1494	
16	1	45	6		36	57·3	0·1245	9·7962
20	1	42	50		38	25·0	0·0941	
24	1	40	23		39	38·9	0·0568	9·7188
28	1	38	22		40	26·0	0·0111	
May 2	1	38	8		40	28·4	9·9556	9·6796
3	1	38	33		40	19·5	9·9401	
4	1	39	14		40	5·9	9·9238	9·6804
5	1	40	14		39	47·4	9·9069	
6	1	41	34		39	23·5	9·8894	9·6858
7	1	43	16		38	53·6	9·8711	
8	1	45	23		38	17·2	9·8522	9·6955
9	1	47	57		37	33·8	9·8327	
10	1	50	59		36	42·5	9·8125	9·7087
11	1	54	31		35	42·6	9·7917	
12	1	58	36		34	33·4	9·7704	9·7248
13	2	3	15		33	13·8	9·7487	
14	2	8	29		31	42·6	9·7266	9·7429
15	2	14	20		29	58·4	9·7042	
16	2	20	49		28	0·3	9.6818	9·7624
17	2	27	56		25	46·9	9·6594	
18	2	35	41		23	16·9	9 6374	9·7828
19	2	44	5		20	29·1	9·6164	
20	2	53	8		17	23·0	9·5964	9·8035
21	3	2	48		13	58·5	9·5781	
22	3	13	3		10	16·3	9·5619	9·8242
23	3	23	51		6	18·4	9·5485	
24	3	35	8	N.	2	7·9	9·5383	9 8448
25	3	46	52	S.	2	10·8	9·5318	
26	3	58	59	S.	6	32·5	9·5291	9·8648

Taking the comet's brightness of December 5 (log r = 0·4301, log Δ = 0·2413) as unity, the brightness of May 2 will be 118; of May 14, 253; of May 26, 359.

Dr. H. OPPENHEIM.

Berlin, 1886, February 18.

Lord Crawford's Observatory,
Dun Echt: 1886, February 24.

ASTRONOMICAL OCCURRENCES FOR APRIL, 1886.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Mars.
Thur	1		Sidereal Time at Mean Noon 0h. 39m. 4·55s.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	11 9 11 24 13 24 13 40	10 0·3
Fri	2		Sun's Meridian Passage 3m. 35·58s. after Mean Noon	1st Oc. D. 2nd Oc. D. 1st Ec. R. 2nd Ec. R.	8 20 9 23 10 50 50 12 42 47	9 55·6
Sat	3			1st Tr. E. 1st Sh. E.	7 50 8 9	9 51·0
Sun	4	2 30 12	● New Moon Conjunction of Moon and Mercury 6° 25′ N.	3rd Sh. I. 3rd Tr. E. 3rd Sh. E.	7 50 9 21 10 47	9 46·4
Mon	5			4th Sh. I. 4th Sh. E.	11 49 13 38	9 41·8
Tues	6					9 37·3
Wed	7			1st Oc. D.	15 39	9 32·9
Thur	8	5 8 5 54 16	Occultation of α Tauri (1) Reappearance of ditto Inferior conjunction of Mercury and Sun	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	12 54 13 19 15 9 15 35	9 28·5
Fri	9	11 49	Occultation of 130 Tauri (6)	1st Oc. D. 2nd Oc. D. 1st Ec. R. 2nd Ec. R.	10 5 11 39 12 44 46 15 19 30	9 24·1
Sat	10	10 25 11 18 0	Occultation of 26 Geminorum (5½) Reappearance of ditto Conjunction of Moon and Saturn 4° 24′ N.	1st Sh. I. 1st Tr. E. 1st Sh. E.	7 48 9 35 10 4	9 19·8
Sun	11	8 44	☽ Moon's First Quarter	2nd Tr. E. 2nd Sh. E. 3rd Tr. I. 3rd Sh. I. 3rd Tr. E. 3rd Sh. E.	8 48 9 49 9 51 11 49 12 41 14 45	9 15·5
Mon	12					9 11·3
Tues	13	8 0	Near approach of ξ Leonis (6) Saturn's Ring: Major axis=40″·20 Minor axis=17″·98			9 7·2
Wed	14	10 57 11 40 13	Occultation of 48 Leonis (6) Reappearance of ditto Conjunction of Moon and Mars 3° 44′ N.			9 3·1
Thur	15	9 21 9 55	Occultation of τ Leonis (5) Reappearance of ditto Illuminated portion of disc of Venus=0·411 Illuminated portion of disc of Mars=0·943	1st Tr. I. 1st Sh. I.	14 39 15 14	8 59·0

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	16	6 14 7 4 9 58 10 44 0	Occultation of 13 Virginis (6) Reappearance of ditto Occultation of URANUS Reappearance of ditto Conjunction of Moon and Jupiter 0° 29′ N.	1st Oc. D. 2nd Oc. D. 1st Ec. R.	11 49 13 57 14 38 50	Mars. — 8 55·0
Sat	17		Sidereal Time at Mean Noon 1h. 42m. 9·39s.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	9 5 9 43 11 21 11 58	8 51·0
Sun	18	2 59 9 5 9 48	○ Full Moon Occultation of κ Virginis (4½) Reappearance of ditto	2nd Tr. I. 1st Ec. R. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E. 3rd Tr. I.	8 18 9 7 21 9 36 11 4 11 23 13 13	8 47·1
Mon	19		Sun's Meridian Passage 0m. 56·71s. before Mean Noon			8 43·2
Tues	20					8 39·4
Wed	21					8 35·6
Thur	22			3rd Ec. R.	8 43 44	8 31·8
Fri	23			1st Oc. D.	13 35	8 28·1
Sat	24			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	10 52 11 37 13 7 13 53	8 24·4
Sun	25	17 15	☾ Moon's Last Quarter	2nd Tr. I. 1st Ec. R. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	10 35 11 1 34 10 12 13 22 14 58	8 20·8
Mon	26			1st Sh. I.	8 22	8 17·3
Tues	27	15 38 16 42	Occultation of θ¹ Aquarii (6) Reappearance of ditto	2nd Ec. R.	9 52	8 13·7
Wed	28	15 8	Occultation reappearance of 78 Aquarii (6)			8 10·2
Thur	29	17	Conjunction of Moon and Venus 0° 19′ N.	3rd Oc. R. 3rd Ec. D. 3rd Ec. R.	9 23 9 59 12 41 19	8 6·8
Fri	30			4th Oc. D. 4th Oc. R.	8 33 10 1	8 3·4
MAY Sat	1	13	Conjunction of Moon and Mercury 0° 6′ S.	1st Tr. I. 1st Sh. I.	12 39 13 32	8 0·0

Mr. Westwood Oliver, with the assistance of Messrs. E. W. Maunder, F.R.A.S., W. F. Denning, F.R.A.S., T. E. Espin, F.R.A.S., A. Cowper Ranyard, F.R.A.S., T. Gwyn Elger, F.R.A.S., J. E. Gore, F.R.A.S., J. Rand Capron, F.R.A.S., Howard Grubb, F.R.S., F.R.A S., W. S. Franks, F.R.A.S., T. W. Backhouse, F.R.A.S., and other well-known observers, has in preparation a practical manual of "Astronomical Work for Amateurs," the aim of which is to help the possessors of limited instrumental means to turn their attention to astronomical researches of real scientific utility, special attention being directed to the comparatively new fields of spectroscopy and celestial photography. The book will be published by Messrs. Longmans & Co. Mr. Oliver invites suggestions from practical workers, which may be sent to him at Lochwinnoch, Scotland.

Books received :—Astronomische Nachrichten.—2,713-14. **Revista de Observatorio Imperial. No. 2. Do.—Bulletin Astronomique sous les auspices de L'Observatoire de Paris.** Tom. III. February, 1886. Gauthier Villars.—Micrometrical Measures of Gaseous Spectra. By Professor Piazzi Smyth. Royal Observatory, Edinburgh.—Solar Spectrum in 1884. By Professor Piazzi Smyth. Royal Observatory, Edinburgh.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1885.

Bridson, J.
Dallmeyer, J. H.
Erck, Dr. W.
Hederstedt, H. B.
Thomas, J. H.

To June, 1886.

Lancaster, I. L.

To Dec., 1886.

Cooke, I. S.
Herschel, Professor A. S.
Hunt, G.
Lamb, Dr.
Lee, J.
Redpath, H. S.
Stothard, Dr.

To Jan., 1887.

Hutchings, Rev. R. S.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

***Post Office Orders* for *the Editor* are to be made payable to JOHN C. JACKSON, Chief Office, London.**

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 281. MAY. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1886—87.

The April meeting was held at the Society's Rooms, Burlington House, on the evening of Friday, April the 9th.

J. W. L. Glaisher, Esq., *President*, in the Chair.

W. H. S. Monck, Esq., 13, Belvedere Place, Dublin, was duly elected a Fellow of the Society.

Col. Tupman read the minutes of the previous meeting, which were confirmed. He stated that the number of presents received since the last meeting was 54. Amongst those calling for special mention are, first, a series of careful drawings of Jupiter, sent by Mr. Thorpe, of Adelaide. There is also a catalogue, sent by Mr. Franks, of 1,730 coloured stars; it contains his own observations as to colour. He gives the name of the star, number in the B.A.C., R.A., declination, magnitude by Heise, and date of observation. There has been presented to the Society, by Mr. Prosser, an old legal document with the signature of Mr. Chester Moor Hall, of Todmorden. He was the inventor of the achromatic telescope, a quarter of a century before Dollond. There has also been presented to the Society a series of four drawings of Jupiter, one of Saturn, and one of the great nebula in Orion, by Mr. Tarrant, of Pinner.

The President: I would ask the meeting to return thanks to the donors of the 54 presents, and especially to Mr. Franks, for the catalogue of coloured stars, and to Mr. Tarrant and Mr. Thorpe for their drawings, as well as to Mr. Prosser for the autograph of Mr. Chester Moor Hall. As Mr. Chester Moor Hall's connection with the achromatic telescope is not generally known, I think Mr. Ranyard would perhaps tell us something about it, which will make the autograph more valuable.

Mr. Ranyard said: I was not prepared to be called upon, but the facts are very simple. Some years ago very little was known about Moor Hall. It was known that about seven years after the patent for making achromatic object-glasses was granted to Dollond, his claim to the invention was disputed by other instrument makers, amongst them by a Mr. Champness, an instrument maker of Cornhill, who began to infringe the patent, alleging that John Dollond was not the real inventor, and that such telescopes had been made 25 years before the granting of his patent by Mr. Moor Hall. John Dollond, to whom the Copley medal of the Royal Society had been given for his invention, was then dead, and his son brought an action for infringing the patent against Champness. There is no report of the case, but the facts are referred to in the reports of subsequent cases. It appears that workmen who had been employed by Mr. Moor Hall were examined and proved that they had made achromatic object-glasses as early as 1733. Dollond's patent was not set aside, though the evidence with regard to the prior manufacture was accepted by Lord Mansfield, who tried the case, as having been satisfactorily proved. The facts are rather mysterious, for Moor Hall was alive at the time of the action, but he does not seem to have been called as a witness. Some few years ago I put together all the facts that I could get at with respect to Mr. Moor Hall, and Mr. Prosser, the gentleman who has given this document to the Society, wrote a letter to a Colchester paper asking if any local antiquary knew anything about Mr. Chester Moor Hall, and the result of his letter was that several letters appeared in answer, and it was shown that Mr. Hall was a bencher of the Inner Temple, and was alive at the time of the action. He was a man of some property, and is spoken of on his tombstone as an excellent lawyer and mathematician. He was not a Fellow of the Royal Society, but must certainly have known of the gift of the Copley medal to Dollond. It is very curious, the conflicting evidence we have to reconcile, but I think the balance of evidence is in favour of there having been a prior invention of achromatic object-glasses before the date of Dollond's patent. The making of achromatic telescopes was a very important gain to astronomy; in some ways even more important than the discovery of the telescope by Galileo. The achromatic telescope enabled a greater step in advance to be made in exact observation than the discovery of the non-achromatic telescope.

Mr. Knobel: Is there any object-glass known to be in existence which was made by Mr. Hall?

Mr. Ranyard: No, not that I know of; but there was one in existence in 1790. It is mentioned in the *Gentleman's Magazine* as being in the possession of a Rev. Mr. Smith, who lived in Charlotte Street, Rathbone Place. This one which was in existence at the end of the last century may still be in existence. It is spoken of as being of a very short focus, 2½ inches by 20 inches focus, and it probably had a triple object-glass such as was not made at first by Dollond. A short-focused object-glass of three lenses could be made with a better correction than two lenses.

Mr. Chambers: I rather think that materials exist for carrying this matter a little beyond the stage indicated by Mr. Ranyard. I forget the occasion of it, but I had occasion some years ago to make some enquiries, and I think I ascertained that Mr. Hall's property, which was near Harlow, in Essex, was in the hands of Mr. Perry Watlington, who was member for South Essex, and that his attention having been drawn to Mr. Hall's antecedents, he succeeded in raking up some information, but what it was I do not remember.

Mr. Ranyard: I had some correspondence on the subject, but I could not hear of any papers having been left.

The President: I think this is a subject of peculiar interest just now, when there seems to be a hope of further improvement in the achromatic telescope.

A vote of thanks was passed to the donors.

Mr. Knobel read a paper by Mr. David Gill, *On some suggested improvements in the practical working of M. Loewy's New Method of Determining the Elements of Astronomical Refraction.* The paper commenced by a statement of M. Loewy's method, and a description of the way in which a prism 45° is mounted in front of the object-glass so that light from two stars at a distance of about 90° away from one another can be brought into the telescope, and their images examined side by side. The distance between the two images would vary with refraction as the lower of the two stars is carried down to the horizon. According to M. Loewy's scheme, the tube of the telescope is to be fixed and the prism made movable with respect to it. Mr. Gill suggested that it would have been better to have the prism rigidly attached to the objective, and the micrometer threads parallel to the line of intersection of the reflecting surfaces of the prism, and the whole to be capable of rotation round an axis. Supposing two stars to be brought into the field of view, then, by a very small movement of the position circle, the images could be brought between the position wires, which marked the direction of measurement, and measures could be made by movable wires with the greatest facility. The altitude of stars could be deduced by noting the time of each observation. The only objection to this method is that angles exceeding ten minutes of arc could not be measured by the spider line micrometer, with sufficient accuracy, when both wires are bisected; but this objection might be overcome by the use of two eye-pieces. The investigation of the elements of refraction on the preceding plan, would involve special apparatus. The heliometer might be adapted for the purpose, but in that case, angles smaller than twice the angles of the prisms could not be satisfactorily measured. When stars differed much from 90 degrees, it would be necessary to use diaphragms on both sections of the object-glass. Accurate observations might be secured of stars of the sixth magnitude, within 88 or 90 degrees of distance.

The President: If I followed clearly the description of M. Loewy's apparatus, the essential point seems to be that it is a purely differential instrument, and therefore free from systematic errors. I was not able to follow so clearly the account of Mr. Gill's modifications of it, but I have no doubt any modifications proposed by a practical observer like Mr. Gill will be very valuable.

The Astronomer Royal: I was hardly prepared to offer any remarks on an instrument like this, which must depend very much for its success upon its practicability. The question seems to me whether it can be practically made useful in determining the co-efficient of refraction. I think the original principle of M. Loewy is very ingenious, and a pretty application of optics; but there seems to me to be a great difficulty in the calculations that would be necessary. There would be a great deal of labour in deducing from each observation the actual refraction. That is the difficulty I should foresee in the application of the method. One difficulty, at any rate, is in the choice of the pair of stars, which may lead to other difficulties. I think it is a principle that is well worthy of being tried. As regards Mr. Gill's modification of it, the difficulty that only one quarter of the object-glass is available for each star seems to me rather a serious one; but I should not like to give any opinion upon it without having an opportunity of examining the paper and considering the matter thoroughly. There is one point that strikes me about it which is worth bearing in mind, and that is there is a very large element of uncertainty in the determination of refraction, arising from the accidental errors due to irregularities in the state of the atmosphere, temperature and barometric pressure being subject to variations at different parts of the course traversed by the ray near to the horizon. One must consider all this in order to get a proper determination of the co-efficient. Near the horizon a large number of observations will be necessary. Therefore a very important factor in determining the law of refraction is that you should be able to get a large number of observations with as little labour as possible. That, I think, is a point which ought to be borne in mind in considering the practicability of this method.

A vote of thanks was passed to Mr. Gill for his paper.

Mr. Common described a paper he had written *On the nebulæ in the Pleiades*. He said that the MM. Henry, who have recently sent photographs of the Pleiades to the Society, and whose communication had appeared in the *Monthly Notices*, had obtained in the photographs three sets of nebulæ which they supposed no one had seen before; but on the 8th February, 1880, he had observed—according to a note which he made at the time—a nebula near Merope. The note he made ran thus: "There is a strong suspicion in my mind of more nebulæ here." His observation was made with the 3-foot reflector; and the position of the stars, and nebulous appearances was as he would show on the black-board (diagram). There appeared indications of nebulæ near Merope, in the direction of Electra; but the photograph of MM. Henry did not show nebulæ at that particular spot, though what he saw was remarkably bright. Yet MM. Henry did show in the photographs a continuation of the nebula towards Electra, and their photograph was certainly a very remarkable one if it was considered how many observers had been watching the Pleiades all over the world with the view to find anything which has not been seen before. In the case of eye observations a bright star usually precludes the possibility of the eye detecting any faint object near it, and to that was probably due the fact that many observers looking near these bright stars had been unable to detect the ray from Electra and Maia; but the photographic plate was not disturbed by the brightness in the neighbourhood, and the power of photography to discover a faint point near a bright point was not at all impaired as in the case of the eye. The remark he made in the note of 1880 as to a suspicion of nebulæ near Merope, extending in the direction of Electra, had no reference to what might occur in the future; and at the end of the note in that year he had made the remark that there was a good deal yet to be settled as to the extent and number of the nebulæ in this cluster.

Mr. Knobel: Mr. Common says that he finds an extension of the nebula in the neighbourhood of Merope towards Electra, but the Brothers Henry have photographed the nebula, extending from Electra towards Merope, which is a very different thing.

In this photograph we have the nebula round Merope, which Mr. Common has observed. We have also a nebula photographed near Electra, which is very much fainter. I do not see in Mr. Common's observations in 1880, that he has recorded the existence of this nebula. I do not understand him to say that he really observed that nebula; the language in his paper seems to be totally different. As to the nebula near Maia, Mr. Common has depicted it as curving in exactly the opposite direction to that which the Brothers Henry photographed it. Those are the points which are different. Another remarkable thing is that the nebula in the neighbourhood of Maia, is photographed so very much stronger than the nebula near Merope, which has been observed by a great many observers, while others have not been able to see it at all. It is evidently not so strong in photographic power as the nebula near Maia; and it would appear from this photograph which the Brothers Henry sent me, that the nebula near Electra is stronger in photographic power than the portion of the nebula which Mr. Common has seen with his 3-ft. telescope.

Mr. Ranyard: I can hardly follow Mr. Common's reasoning as to the light of nebulæ being overpowered by the brightness of the light dispersed in the atmosphere. As far as I am aware, no photograph has yet been taken which shows traces of the photographic action of this dispersed light, though it is evident to the eye; giving an appearance which has been frequently described as like sunrise before a large star enters the field of view. I think that it seems to be more probable that the reason why these nebulæ have not been seen by observers is that they shine with light which, though active for the photographic plate, is at the violet end of the spectrum, where the eye is little affected. Even the plates of the Brothers Henry do not show any trace of the photographic action of the light dispersed in the atmosphere around bright stars; and if it is true that photographic plates do not register slight differences of brightness which are easily seen by the eye, as I endeavoured to show a few meetings ago, Mr. Common's explanation cannot be the true one. I had a lens of uranium glass made, and also a cell containing esserine, an extract from the Calabar bean, which degrades light of short-wave length, thinking that possibly I might be able to see the nebula about Maia if it was shining with light in the H and K region of the spectrum, by using a screen of such fluorescent substances; but the idea did not strike me sufficiently early, and I have not had a satisfactory view of the Pleiades since, and must now wait till next season. One needs a very clear night. But possibly some fluorescent substance that degrades the wave-length, may bring the light of these nebulæ into the visible part of the spectrum, and enable us to see them, though we cannot under ordinary circumstances.

Mr. Waters: I think Mr. Ranyard's remarks are probably entirely right in reference to the matter, but it might possibly be set at rest by the use of different plates. If you take some of those plates which Captain Abney has been using they might possibly solve the question. The nebula possibly emits only rays of wave-lengths, which cannot be seen, but on the plates different amounts would be photographed.

Mr. Common: I should like to mention one or two points as to the exact position of the nebulæ. In dealing with a bright star you must take it as you see it in the eye-piece. You would find that before you get to clear dark sky the star must be quite out of the eyepiece. On that point depends the whole of this question as to the photographing of these nebulous masses near bright stars. As long as the eye can get away from a bright star it can see masses of light, simply because those masses of light are in contrast with the dark sky beyond. Here they are in contrast with the illuminated part of the atmosphere, which every one sees when a bright star is coming into the field; before one sees it there is a ring of brightness visible.

Mr. Ranyard: No one has ever photographed that patch of brightness.

Mr. Common: It is easy enough to do; if you use a reflecting telescope and use three bars across, you will find a very pretty exhibition of the six rays. You see it in my photograph of the Orion nebula.

Mr. Ranyard: I take those discs to be due to photographic irradiation.

Mr. Common: With regard to that particular matter of seeing a faint point of light in the presence of a bright one, I had the curiosity to look lately at the Pleiades, to see if I could with the 6-inch telescope the fainter larger nebula. The zodiacal light, which you know at this time of the year extends up a considerable distance about the Pleiades group, when it is well visible, the light from the zodiacal light was sufficient to overpower any nebula light. All I want to point out is the remarkable fact that the Brothers Henry should have sat down to photograph, as I have no doubt they did, a nebula that was exceedingly faint in the Pleiades group, and ending by photographing three nebulæ that no one had hitherto seen except myself. I saw one of them, but it was quite by chance that I recorded it. If one only knew what was going to happen, one would be more careful to assume quite a different rôle. If I had known the importance of it, I should have looked again and again, and should have been able to have said there was a nebula near Merope.

The President: We have the pleasure of seeing Lord Rayleigh here, and perhaps he will be able to tell us what is known in regard to the question raised by Mr. Ranyard and Mr. Waters, as to the wave lengths.

Lord Rayleigh: I would rather be inclined to ask questions than answer them. It would interest me very much to know what would be the explanation of these very large discs we see produced on the photographic plates by the brighter stars—whether there is any accepted theory to account for them. I used to photograph in old days, but I am not accustomed to the gelatine plates which have superseded all others. I think it may be some peculiarity in them. As far as I have had an opportunity of examining stellar photographs, the edge of the disc is always very fairly sharp. I have a vague idea that they are not due to light generally diffused and gradually falling away; they suggest some different explanation. As to the kind of light that comes to us from these very faint nebulæ it would be a difficult question to decide. It is quite possible that in these nebulæ, the photographic intensity may vary as compared with the luminous intensity, and that might be an explanation, but I should rather be inclined to look for an explanation, as Mr. Common has suggested, in the difference of the behaviour of the eye, and the photographic plates in the neighbourhood of more intense sources of light.

Captain Noble: There is only one point I wish to mention with reference to the rays of different refrangibility emanating from the different nebulæ. As a matter of fact, every practical observer knows that blue stars when small are visible much more readily than red and orange. If, say, you lose in the telescope an average star of the eleventh magnitude, you will lose an eleventh magnitude red star before you lose a blue star. Therefore, it may well be that the colour of a nebula has a good deal to do with its actinic power, or capability for being reproduced in a photograph.

A Member: I could draw the attention of the Society to a letter which was inserted a little while ago in the *Comptes Rendus*, by M. Wolff, on this very point; and he dwelt very strongly upon the necessity not only of photographing at all times in exactly the same way, but he pointed out that a change in the preparation of the plate and in the mode of development, and almost any change you could make in photography with regard to definition, would make a change in the photographic record; and it is very necessary if you are to have strictly comparable photographs that you should always have them taken in the same way and by the same process and mode of development. That being the case, you should be very cautious as to how you assert that anything seen by the eye, and not recorded on the photographic plate, is due to change. He has himself pointed out in these particular photographs of the Brothers Henry that there are certain stars absent and certain nubulous masses present, and it is quite impossible that those could exist to the eye which are not visible on Henry's plates unless there is some difference between the impression on the plate and the impression on the eye.

Mr. Waters: I should like to point out that if you take a photograph of any number of bright stars, the apparent magnitude of the stars as seen and on the photographs are entirely disproportionate. Small stars occasionally appear as stars of a higher magnitude in the photograph, whereas to the eye they are of much smaller magnitude and this may be the case with the nebulæ. The nebulæ may not be visible to the eye, and may be photographed possibly with great ease owing to the greater actinic power their light emits.

Mr. Knobel: When Dr. Gould was here he exhibited a photograph of the cluster round κ Crucis, and pointed out the great difference of photographic magnitude due to colour.

Dr. Spitta: With respect to the diffused light before referred to and the dark ground of Henry's photographs, it may be due to the fact that they used an achromatic telescope. I always fancy the field in the achromatic is very much darker than in the reflector. When I was last in Paris I spent the evening with the Brothers Henry, and used their equatorial, and they pointed out that the star discs were very sharp with that instrument, which is a compound one; and when I looked in my own reflector at home, I saw that the diffused light was much greater; but in using the refractor you do not notice the illumination about bright stars anything like so much.

A Member: I have observed the Pleiades, and have noticed that round Maia there is apparently a considerable amount of nebulous matter; and throughout the whole cluster there is a large amount of light. I remember four or five years ago Mr. Common and I had some discussion upon the subject in this Society, and Mr. Common indicated what he has shown on the black-board now. But he has not indicated what I have seen. It is something like that oval patch he has drawn, but there is a large amount of light which he does not show. Around Merope there is a beautiful oval light, which I have seen constantly, and I have heard over and over again that this is variable. I believe it is only variable from the state of the atmosphere, and not otherwise. Round Maia I have seen a considerable amount of light; throughout the whole constellation is full of light, and a large amount of what I call nebulous matter, but there is no resemblance there between what Mr. Common has shown and what I have seen with my telescope; and if you will refer to Mr. Hall's paper on the subject you will see the appearance that I saw. Mr. Hall received from me a drawing which I made for Argelander's Map. I presented him with this drawing, and what he has published is very like what I gave him.

Mr. Common: Before the discussion on this matter ends I should like to point out this fact: every photographer knows that by so adapting the exposure, or time of exposure, of the plate, he can get either a hard black and white picture, or a picture which he calls a finely graduated picture, showing half lights. Nothing is easier than to take a picture showing first a bright disc, without graduation at the edges, or what is called density of image. With a short exposure, and a long development, you get a sharp picture of this; and I think it is no reflection upon the Brothers Henry that in their enlargements they have been able to reduce the stars to bright points without any shade.

A vote of thanks was then passed to Mr. Common for his paper.

Mr. H. H. Turner read a paper *On Observations for Coincidence of the Collimators through the Cube of the Transit Circle, at the Royal Observatory, Greenwich.* The central cube of the transit instrument at Greenwich was pierced in December, 1866, by the direction of Sir Geo. Airy, with a number of sector-shaped holes, so that one collimator can be used on the other without removing the instrument. It was found that a slight correction amounting to about half a second of arc was necessary to make the coincidence with uninterrupted view accord with the coincidence obtained through the cube of the transit. Mr. Turner has been experimenting on the cause of this want of coincidence by means of a wooden model which could be placed in the position of the cube, when the instrument was raised. The same want of coincidence was observed, and he came to the conclusion that the disturbance was due to the want of symmetry of the object-glasses of the collimators. The mean of the readings came out with the metal cube ·501 of a revolution; with the wooden cube ·504; and with uninterrupted vision ·465.

The President: I would invite remarks on this important paper on what seemed to be very extraordinary discrepancies. I was not prepared for the result obtained with the wooden model. It is satisfactory that the cause of the discrepancy has been settled.

The Astronomer-Royal: I must say I feel it is a very satisfactory result Mr. Turner has arrived at. This has been a very puzzling discordance, and looking at it I could not see how one could account for it optically. As is the usual case with astronomers when in a difficulty about instrumental discordances, I was disposed to attribute it to differences of temperature. I think Mr. Turner has shown that it has nothing to do with temperature, and that it is purely an optical effect arising from the circumstance that a portion only of the object-glass is used in one observation, and in the other we use the whole of the object-glass. This paper, which is on a rather technical matter relating to the Greenwich transit circle, has a much wider application than might be supposed at first sight, because it will apply to a great number of observations in which it is assumed, when we use a certain portion of the object-glass in one observation, and the whole of it in another, that the observations are strictly comparable.

Lord Crawford: Have any careful observations been made with the object-glasses of these collimators? As a rule the makers think a collimator object-glass may be figured any way, so that it will collect light. Is there anything specially wrong in the figures of these lenses? The figures might be annular or wavy in section.

The Astronomer-Royal: I think it is quite true you might get a difference of that kind, but I believe these object-glasses are very fair in figure, and as far as I have been able to try them, they give very good definition, but I do not think they have been tested in star observations thoroughly.

Lord Crawford: The object-glasses at Greenwich are exactly the same as mine, and the collimators are precisely the same as the collimators of my transit circle; I think mine are up to star work, and they are fair object-glasses of the first-class.

Mr. Common: It would be very interesting to know if any experiments were made with the upper half and with the lower half shut out, and *vice versâ,* and the readings taken with each.

The Astronomer-Royal: I think I may say with regard to the question of bad definition of the object-glasses that it is a very large quantity we are dealing with, the discrepancy being half a second of arc, so that it can hardly be considered a quantity of the same order as is involved in faulty definition of the object-glass. The object-glass gives a good image of the wires. Many further experiments might be tried in this direction. I think Mr. Common's suggestion is a very good one, that something more should be done.

Mr. Dunkin: The experiments made by Mr. Turner are very interesting, and so much so that one would like to know whether in the Cape transit circle, where there is no interference in the aperture, there is any difference between the readings when the instrument is raised, and when the wires of the south or north collimator are viewed through the cube. I do not know whether that has ever been determined.

Mr. Turner: I asked Mr. Finlay, when he was over here, if he had noticed anything of that kind, and he said nothing of that kind had been noticed. At Cambridge, also, there has been nothing of the kind noticed.

Lord Crawford: We have not anything of the kind at Dun Echt.

Mr. Common: There is one point with regard to the transit circle at the Cape. There is a hole pierced through the cube, but is that the same size as the collimators? I rather fancy it is not, and certainly it is not the same size as the object-glass of the transit.

The President: I should like to ask Mr. Turner whether there is any other instrument which has a similar arrangement.

Mr. Turner: There is a slight want of symmetry in a vertical direction, but none in a horizontal direction, in which direction the defect of collimation takes place. In the flexure observations in a vertical direction, the want of symmetry does not seem to affect the matter.

The President: I think that Mr. Turner misunderstood me; I asked him if there was any other large transit instrument which has the same arrangement, in which the orifice has these cross bars in it as at Greenwich, or is it quite unique?

Mr. Turner: I think it is quite unique. I have never heard of any other similar arrangement.

Lord Crawford: I have seen several, but never seen one like that.

The President: And I don't suppose it is likely any other will be made.

A vote of thanks was passed to Mr. Turner for his paper.

Mr. Knobel read a paper by Mr. Trouvelot, on the *Protuberances visible in the Spectrum with a Narrow Slit.* The author referred to a paper on the subject read by Mr. Maunder at the last meeting. He had only seen the report in the *English Mechanic,* where Mr. Maunder was reported to have said that the observation recorded by him was impossible. He understood Mr. Maunder to say, that the observation was impossible, because the protuberances of such a height—say three minutes'—could not be seen with the slit open, and because prominences could not be seen at all with a narrow slit. Such observations by an experienced observer like Mr. Maunder had excited his surprise;

because the first phenomenon was not so difficult to detect, and the second might be seen once or twice in a year. Protuberances on the spectrum might not have been seen by Mr. Maunder, as they were rare, but he (Mr. Trouvelot) could easily see protuberances of three minutes in height with a wide open slit. When the sky was sufficiently clear, and the prominence tolerably bright, he used a tangential wide open slit, which permitted a sight of the whole form at a glance. This brightness diminished with the width of the slit. If the observer covered his head, and allowed his eye to become accustomed to the darkness, he would not be long before he saw, with the wide-open slit, a bright well formed protuberance, which could be seen also as the slit was narrowed, and almost closed. The paper was accompanied by a movable diagram showing how the prominences had been observed when the slit was drawn away from the sun's limb.

Mr. Maunder : It is rather a pity that M. Trouvelot wrote his answer without waiting to see my paper. He took the account from that given in the *English Mechanic*, which gave a very condensed report of what I said, and unfortunately omitted the gist of the whole matter. My criticism of M. Trouvelot's earlier observation was based on this point, that his observation was not reconcilable with the theory that displacement of the lines of the prominence indicated motion in the line of sight, because so interpreted his observation would mean that the prominence was moving, not only at the speed he mentions—namely, up to about 1,600 miles per second, but with every conceivable variety of motion from rest up to that rate of motion. We cannot conceive the same body at the same time at rest and in rapid motion, and not only that, but in every sort of motion. Therefore, I thought it was proof positive either that we must modify that theory, or we must suppose there was some error in the observation. The further information which he has now sent seems to make it more clear than it was before that the error lies in the observation. He describes a prominence, practically a straight one about two minutes in height, and he sees it first with a narrow tangential slit, close to the limb. He sees the *entire* prominence whilst he has the narrow slit close to the limb. He then moves the slit away from the limb, parallel to the tangent to the sun's limb, and as he moves it away the prominence gradually disappears on the side nearer the red end. It is not affected at all with regard to the part which is projected on the spectrum towards the violet end. He moves his slit little by little until it is moved two minutes away from the sun's limb, and then he sees just the very top of the prominence. Of course it is absolutely impossible that he could get any light in the spectrum, except what he received through the slit, unless he had a transparent slit-plate, and the only way of accounting for observation as it is described would be to suppose that, whereas the slit-plate towards the red was opaque, the slit-plate towards the violet was transparent. The probability is that he was not accurately informed either as to the width of the slit he was using, or as to the scale from the sun's limb, so that, whereas he thought he had a narrow slit, he had a wide one; and whereas he thought he was observing a prominence of two minutes, he was observing one very much smaller. If we make that supposition, the observation is intelligible; but I do not see any other way of explaining it. It is clearly impossible he could see lofty protuberances through a narrow slit close to the sun's limb, or that he could see light that was two minutes away from that slit. (Hear, hear.) I have seen prominences projected on the spectrum, but it has been under very different circumstances. Such observations are indeed to be found in all the text-books, and have frequently been made. I have made them with a radial slit; but in this case the displacement implied that the entire mass of gas was moving at the same speed. There is nothing self-contradictory in that; but an observation like M. Trouvelot has described is self-contradictory, as he observed the prominence in rest and in rapid motion at the same time.

Col. Tupman: If the observation cannot be accounted for on the displacement theory, it means that he saw the form of the prominence through one of the jaws of the slit of his spectroscope—that is, that he saw it through a piece of solid brass.

Lord Crawford: The observation as described by M. Trouvelot is evidently impossible; but I have frequently seen the displacement of a bright prominence line with a narrow slit, which looked like a prominence form projected on the spectrum with a floating cloud or detached portion of a prominence separated from the sun's limb. This is very possible; but you could not move the slit and see the prominence gradually swallowed up.

The following papers were either partly read or taken as read:—

W. F. Denning: *Note on the Radiation of Meteors.*

E. B. Powell: *On the Orbit of α Centauri.*

Adelaide Observatory: *Observations of the Phenomena of Jupiter's Satellites made with the 8½-inch Cooke equatorial in the year* 1885.

W. H. Finlay: *The Magnitude of η Argus, in March,* 1886.

Radcliffe Observatory, Oxford: *Observations of Occultations of Stars by the Moon,* 1884-85, *and of Eclipses of the IVth Satellite of Jupiter,* 1885.

W. G. Thackeray: *On the semi-diameter of Venus.*

Royal Observatory, Greenwich: *Observations of Comet* d (*Fabry*) *and* e (*Barnard*).

The meeting adjourned at ten o'clock.

LIVERPOOL ASTRONOMICAL SOCIETY.

The second session of the educational branch was closed on the 21st April, by a lecture on spectrum analysis, by Mr. James Gill, of the Sailors' Home. Borrowing an illustration from sound waves, the lecturer described how the waves of light could actually be measured, and the nature of their propagation ascertained by simple arithmetical formulæ. The difference between the approach and recession of an express train altered the musical pitch of its whistle very appreciably, and the same phenomenon happened with light waves. By the spectroscope they not only recognised the different elements which were present in distant stars and nebulæ; but, by a shifting of the position of the hydrogen line either to the red or violet end of the spectrum, they were enabled to ascertain at what speed a distant luminous object approached or receded from us in the line of vision. The spectra of different metals were thrown upon the screen, and the relative dispersions of light by prisms of different densities were explained. The chairman, Mr. W. H. Davies, F.R.A.S., said Mr. Gill's lecture had afforded an excellent illustration of the difference between a knowledge of dry facts and their scientific application. The system of "cramming" might make them familiar with any number of facts, but unless they learned to apply them the knowledge would be of little practical use. Mr. Gill had, he thought, good reason to be proud of the progress they had made under his teaching, and he hoped next session that the students would themselves undertake the management of sub-branches. The Liverpool Astronomical Society contemplated publishing a monthly magazine of a more extended character than heretofore, and its pages would be open to such of them as would prepare papers on astronomical subjects. It was, the chairman added, his pleasing duty to present Mr. Gill with a massive marble timepiece, which the students hoped he would regard as a memento of the many pleasant evenings they had spent under his tuition. Mr. Gill having suitably acknowledged the testimonial, the secretary, Mr. J. P. Weir, stated that not only had the branch been self-supporting, but they had a substantial balance in hand towards the expenses of next session, which would begin in October. This being all the business, the session was then brought to a close.

SELENOGRAPHICAL NOTES, MAY, 1886.

By Thos. Gwyn Elger, F.R.A.S.

The Ariadæus and Hyginus Clefts. That portion of the great Ariadæus cleft lying east of the mountain range running by Silberschlag towards Julius Cæsar, is seen to the best advantage when the sun is rising on the east walls of Godin and Agrippa, as at this time the undulating and somewhat rugged character of the region north of the latter ring-plain, and the frequent widenings and contractions displayed by the cleft in passing over the inequalities it encounters are apparent without being too much obscured by the neighbourhood of the terminator. Between 8h. and 9h. on April 10th, these conditions of illumination prevailed with a clear sky and favourable definition, permitting the use of a power of 380 on my 8½-in. Calver reflector. The shadows thrown by the Silberschlag mountain barrier were unusually distinct and sharply defined, the most noteworthy being that of the lofty peak β, just south of Silberschlag, which, at 8h., extended in the form of a very acute spire nearly up to a dark-rimmed crater (not shown by Mädler or Neison), some distance north of Agrippa. From this object a narrow valley runs in a winding course to the Ariadæus cleft, meeting it nearly at right angles but not crossing it. Another and deeper valley originates east of the crater and likewise trends to the cleft, which it crosses obliquely some twenty or thirty miles east of the point of junction of the first. Between these valleys there is a wide stretch of upland over which the cleft passes in a somewhat attenuated condition, and at about the loftiest part of the swelling is the point of junction of the cleft (discovered by Gruithuisen, and first seen by Schmidt in June, 1862), which connects the Ariadæus with the Hyginus system. This delicate object was very satisfactorily observed. Down the eastern slope of the upland it was traced as a fine dark line gradually widening as it attained a lower level, and becoming much broader and exhibiting a bright eastern flank till it was lost in shadow near the terminator.* Neison and other selenographers draw the Hyginus cleft as terminating southwards at a crater north of

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* *Note.*—Since writing the above I have received a good drawing of the Ariadæus system, also made on 10th April, by Mr. G. T. Davis, of Theale, with a Wray achromatic, 3¾-in. aperture, in which this connecting cleft is shown very distinctly, running nearly up to the terminator; while the principal cleft east of the junction appears to have been quite inconspicuous.

Agrippa, and Dr. Klein (*Sirius Band,* xvii. p. 6.) has traced it up to the north wall of this formation. On the evening of April 10th, at 8h. 30m., it was followed as a fine dark line crossing the two valleys just referred to as far west as the extremity of the shadow of the peak β, south of Silberschlag, passing in its course a little north of the dark crater just mentioned in connection with this shadow. At 8h. on the following night, April 11th, when definition was also favourable and the morning terminator nearly bisected the ring of Archimedes, it was traced nearly as far west as on the 10th. The crater *m*, north of Agrippa, which, according to Neison and others, marks the southern end of the Hyginus cleft, actually stands some distance south of it, but there is a narrow ridge running from the cleft up to this crater, which at this stage of illumination looks so much like a cleft that it has possibly led observers to mistake it for the extremity of the Hyginus rill, though the latter really extends many miles south-west of the crater in the direction of the gap or pass in the wall north of β. On this evening the connecting cleft between the Ariadæus and Hyginus system was visible as a faint white line, as were also portions of the other branch from the same point of junction on the Hyginus cleft running south of the connecting cleft, though no trace of this object was observed on the previous night, owing probably to its nearness to the terminator. It is no little credit to Schroeter and Gruithuisen that they succeeded in detecting this cleft, which now even in large telescopes is not by any means an easy example of the class. Schmidt shows a break in the connecting cleft near its junction with the Ariadæus system, its place answering as regards position to the crest of the upland between the two valleys I have described. This interval has no real existence, as the cleft is clearly continuous from end to end, though it is contracted to a nearly invisible thread at the place where the break is represented.

The Triesnecker Clefts.—At 8h. 30m. on April 11th the most prominent of these objects were satisfactorily observed, though the terminator, then crossing Archimedes, was somewhat too advanced towards the east for the shallower members of the system to be visible. The cleft from Rheticus A to the bend west of Triesnecker and its northerly extension towards Hyginus *a*, and the branch from it crossing the cleft from the north border of the Rheticus to the bend were especially conspicuous. The arm from Rheticus to the bend, marked ζ in Neison's Map II, is usually described as originating among the mountains lying outside the north border of the ring-plain. Gaudibert, however, has traced it round the east side of the formation, an observation which I was able to confirm satisfactorily on this occasion. Just before reaching the outer barrier of rocks it suddenly turns towards the south-east and cuts through a long spur, running north from the border of Rheticus, a little west of Rheticus B, and then, as a very coarse and obvious cleft, it curves round the eastern slope of the ring-plain, passing over a very rough and uneven region to a long rocky projection jutting out many miles beyond the southern side of Rheticus. Running down the centre of this it was traced far beyond the limits of the ring-plain and its immediate surroundings trending towards Horrocks. Schmidt shows some considerable depressions and crater-rows on the eastern border of Rheticus, but no indications of this cleft.

Catalogue of Lunar Clefts.—As I am preparing for publication a catalogue of lunar clefts, I shall be greatly obliged if observers will favour me with any observations relating to these objects, more especially as regards the Triesnecker-Hyginus Systems.

Kempston, Beds: 20th April, 1886.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

GALILEO'S RANK IN THE MOON.

Sir,—Referring to Mr. Elger's note in your last number, I may be allowed to conclude this subject by again reminding him that no "revision" is involved in my proposal. It is fully doubtful whether a revision will ever be thought practicable. It is a case, and an urgent one, for *addition* as taking Webb's numbers in *Celestial Objects*, 405—490 have been, among which I am rejoiced to see his own honoured name.

I am, &c.,
EDWARD ALLEN.

Castlechurch Vicarage:
April 7th, 1886.

REVIEWS.

Traité de la Lumiere . . . avec un discours de la cause de la Pesanteur. Par M. Christian Huyghens, Seigneur de Zeelhem. Edidit cum prefatione Latina, W. Burckhardt. Leipsic: Gressner & Schramm. pp. iv. 134.

These two works were first printed in French in Leyden, 1690, and afterwards Latinized in the author's "Opera Reliqua." In his preface the editor gives an account of Huyghens, and says that the rarity in these days of the Treatise on Light leads him to think that its reproduction would be welcomed by mathematicians. The dissertation on gravity, which always accompanied it, is also added. The writings of Huyghens, as it has been observed, seem to form the natural step in the chain between those of Galileo and Newton. The reprint of the Treatise by the founder of the undulatory theory of light will probably interest many. M. Burckhardt says he has followed the French edition, and scarcely altered anything, although Huyghens wrote in a careless style.

Nouvelles recherches sur l'Agrandissement apparent des constellations, du soleil et de la lune a l'horizon Par M. Paul Stroobant. Bruxelles: 1885. pp. 17.

The author describes some fresh investigations of this subject, which he thinks tend to confirm his previous conclusions. The mean of the apparent size of an object in the zenith, estimated by eight different observers, was found to be 81·9, taking the horizontal magnitude as 100. We are inclined to acquiesce in the common explanation of this curious optical, or rather mental illusion, that we are accustomed to compare the sun and moon on the horizon with things on the earth, a habit that unconsciously influences us when darkness conceals all terrestrial objects. In his "Astronomy without Mathematics," Sir E. Beckett offers another explanation. He believes the reason is that when things are on the horizon we compare their linear dimensions with the length of horizon which the eye takes in; but that in the middle of the sky or earth we compare the area they cover with the area the eye takes in. Assuming the eye to see distinctly over 30° of apparent width, the moon on the horizon covers one 60th of that; but when it is high up it only fills the 3,600th of the area of sky which the eye sees all around it. Between these two extremes some compromise is made unconsciously, and a different one by different people's eyes. He also remarks that a man looks much larger against the horizon on the top of a hill than when he is viewed at the bottom from the top.

M. Montigny in his report prefixed to this paper in the *Bulletins de l'Académie Royal de Belgique,* whilst commending the investigations of M. Stroobant, observes that the great apparent enlargement of the constellations, especially the Great Bear, near the horizon, in comparison with its appearance in the zenith, is not fully accounted for.

We have received the July, August, and September numbers of the *Bulletin Astronomique,* published under the auspices of the Paris Observatory, edited by M. E. Tisserand, with the co-operation of MM. Bigourdan, O. Callandrean, and R. Radan. This excellent serial, besides papers suited for advanced mathematicians, contains much which interests the amateur. Its reviews and notices of books and periodicals, are extensive and full. In the September number we are informed of the special work which is being carried on in twenty-eight observatories. In the July number, M. Wolf concludes his elaborate series of papers on the different hypothesis of the cosmogony, with a review of the hypothesis of M. Faye. He reserves for a future publication an examination of the researches of Mr. G. Darwin on the formation of satellites, and a review of the various opinions about the end of the worlds. In the numbers for August and September, M. Trouvelot continues his articles on the inner structure of the solar envelope.

Nebula in Andromeda. In a paper, by Flammarion, in *L'Astronomie* for November, it is stated that the new star was not visible on the 15th and 16th August at the Observatory of Florence (Tempel). On the 17th, M. Ludovic Gully, at Rouen, perceived, to his astonishment, that a star appeared in the nucleus. On the 19th, Mr. Isaac Ward, at Belfast, observed in the nucleus a distinct and faint star, which he estimated as of the 9½ mag. The following are its magnitudes at subsequent dates: From 17th to 22nd, 9; from 30th Aug. to 3rd Sept., 6½; Sept. 4th, 7; 7th, 7½; 11th, 8; 15th, 8½; 20th, 9; 27th, 10; Oct. 1st, 10½; 8th, 11; 15th, 11½.

We are very glad to find that the number of amateur astronomers is increasing in France, and an extending interest is taken in the science. Much of this is due to M. Flammarion.

A Popular History of Astronomy during the Nineteenth Century. By Agnes M. Clarke. Edinburgh: Adam and Charles Black. 1885. pp. xiv. 468. Price, 12s. 6d.

The sight of this book was an agreeable surprise to us. After reading a little we were freed from any slight shade of misgiving as to its character, and gave ourselves up luxuriously to the high enjoyments of its perusal. The writer is quite at home in her subject. Extensive reading, good judgment, impartiality, fairness, accuracy, and discrimination, are manifested in this history, which is written in pure and graceful English—a lucid, natural style, frequently adorned with phrases and illustrations of great felicity. There is a freshness and originality in the treatment even of familiar subjects. We gather, as it were, fruit whilst still wet with the dew of morning, and its bloom not rubbed off by any previous fingers. We have learned much from the authoress that we did not know before, and sincerely thank her for a book which fills an important place, and the great merit of which will, we think, cause it to take in its time the place of a "classic" work. The history is brought down to the present year. The discovery of the "star" in the Andromeda Nebula in August last, is alluded to.

Catalogue of 4,810 *Stars for the Epoch* 1850; from Observations made at the Royal Observatory, Cape of Good Hope, during the years 1849 to 1852, under the direction of Sir Thomas Maclean, Kt., F.R.S., Her Majesty's astronomer at the Cape. Reduced and published under the direction of David Gill (Aberdeen and Edinburgh), F.R.S., &c., Her Majesty's astronomer at the Cape. Published by order of the Board of Admiralty, in obedience to Her Majesty's command.

When Mr. Gill assumed direction of the Observatory in June, 1879, he found that about half of the computations to mean place had been made under the direction of Mr. Stone. It was a comparatively small matter to complete this part of the work; it was the final revision, the comparison with other catalogues, and the scrutiny of doubtful observations, that cost most in time and patience. The work of two incompetent assistant observers had to be rejected. Mr. Gill cannot regard the work as a satisfactory one, but he has done his best to utilize the observations on which it is founded, and to eliminate its errors. A comparison afterwards made between the Greenwich and Cape Catalogues for 1850, was satisfactory as far as it went, but raises the important question of the cause of the discordance between the Cape Catalogues for 1850 and 1880, and points, as probable sources of this, to variation in the places of collimation in the Cape Transit Circle at different altitudes, or less probably to lateral refraction from the unsymmetrical arrangement of the neighbouring masonry of the building.

Notwithstanding the drawbacks, it would have been a great pity if this catalogue had not appeared. It *must* have some, perhaps considerable, value, and it is fortunate that its preparation was in the hands of so able and practical an astronomer as Mr. David Gill. May he reap the reward of his pains and labour.

THE PLANETS FOR MAY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	1 4 3	N. 3 49½	8″·8	22 23·0
	9th	1 32 16	N. 6 15	7″·6	22 19·7
	17th	2 11 10	N. 10 13	6″·6	22 27·1
	25th	3 0 31	N. 15 5½	5″·8	22 44·8
Venus ...	1st	23 45 26	S. 2 18	23″·4	21 4·6
	9th	0 16 3	N. 0 20½	21″·4	21 3·7
	17th	0 47 34	N. 3 12½	19″·8	21 3·7
	25th	1 19 58	N. 6 12	18″·4	21 4·5
Mars ...	1st	10 38 37	N. 10 45	12″·2	8 0·0
	9th	10 44 18	N. 9 52	11″·4	7 34·2
	17th	10 52 2	N. 8 48	10″·6	7 10·4
	25th	11 1 27	N. 7 34½	10″·0	6 48·4
Jupiter ...	1st	11 50 26	N. 2 41½	39″·4	9 11·6
	9th	11 48 52	N. 2 49½	38″·6	8 38·6
	17th	11 47 59	N. 2 53½	37″·8	8 6·2
	25th	11 47 48	N. 2 52½	37″·2	7 34·6
Uranus ...	1st	12 16 47	S. 1 0	4″·0	9 37·9
	17th	12 14 14	S. 0 51	3″·8	8 33·4

Mercury rises about half an hour before the sun throughout the month.

Venus rises about an hour and a half before the sun throughout the month.

Mars sets about an hour and a half before sunrise at the beginning of the month, the interval increasing.

Jupiter sets an hour before sunrise on the 1st, the interval increasing.

ASTRONOMICAL OCCURRENCES FOR MAY, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Sat	1	13	Conjunction of Moon and Mercury 0° 6′ S.	1st Tr. I. 1st Sh. I. 1st Tr. E.	12 39 13 32 14 55	— 9 11·6
Sun	2		Sidereal Time at Mean Noon 2h. 41m. 17·69s.	1st Oc. D. 2nd Tr. I. 1st Ec. R.	9 49 12 55 12 55 53	9 7·4
Mon	3	15 42	● New Moon Saturn's Ring: Major axis=39″·00 Minor axis=17″·32	1st Sh. I. 1st Tr. E. 1st Sh. E.	8 1 9 22 10 17	9 3·3
Tues	4		Sun's Meridian Passage 3m. 22·14s. before Mean Noon	2nd Oc. D. 2nd Ec. R.	7 50 12 28 48	8 59·1
Wed	5					8 55·0
Thur	6	8 12 9 3 9 42	Occultation of 111 Tauri (5½) Reappearance of ditto Occultation of 117 Tauri (6)	3rd Oc. D. 3rd Oc. R. 3rd Ec. D.	9 57 12 55 13 57 55	8 50·9
Fri	7	10	Conjunction of Moon and Saturn 4° 13′ N.			8 46·8
Sat	8			4th Tr. I. 1st Tr. I.	14 27 14 28	8 42·7
Sun	9			1st Oc. D. 1st Ec. R.	11 37 14 50 18	8 38·6
Mon	10	14 20	☽ Moon's First Quarter	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	8 55 9 56 11 11 12 12	8 34·5
Tues	11			1st Ec. R. 2nd Oc. D.	9 18 58 10 14	8 30·4
Wed	12	0	Conjunction of Moon and Mars 2° 6′ N.			8 26·4
Thur	13	4	Conjunction of Moon and Jupiter 0° 25′ N.	2nd Sh. E. 3rd Oc. R.	9 24 13 31	8 22·3
Fri	14	14 42	Occultation of θ Virginis (4½)			8 18·3
Sat	15	18	Conjunction of Neptune and Sun Illuminated portion of disc of Venus=0·572 Illuminated portion of disc of Mars=0·900			8 14·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Libræ. —
Sun	16		Sidereal Time at Mean Noon 3h. 48m. 19·12s.	**1st** Oc. D.	13 26	11 6·3
Mon	17	13 47 10 16 11 34	○ Full Moon Occultation of η Libræ (6) Reappearance of ditto	**3rd** Sh. E. **1st** Tr. I. **1st** Sh. I. **1st** Tr. E.	10 37 0 45 11 51 13 1	11 2·3
Tues	18	12 37	Near approach of 24 Scorpii (5)	**1st** Ec. R. **2nd** Oc. D.	11 13 30 12 41	10 58·4
Wed	19		Sun's Meridian Passage 3m. 45·36s. before Mean Noon	**1st** Sh. E.	8 35	10 54·5
Thur	20			**2nd** Sh. I. **2nd** Tr. E. **2nd** Sh. E.	9 12 9 42 11 58	10 50·6
Fri	21	10 57 17 2	Occultation reappearance of *d* Sagittarii (5) Near approach of 6858 (6)			10 46·6
Sat	22					10 42·7
Sun	23		Saturn's Ring: Major axis=38″·10 Minor axis=16″·74			10 38·8
Mon	24			**3rd** Tr. E. **3rd** Sh. I. **1st** Tr. I.	9 58 11 44 12 36	10 34·8
Tues	25	11 36	☾ Moon's Last Quarter	**1st** Oc. D.	9 44	10 30·9
Wed	26			**1st** Tr. E. **1st** Sh. E.	9 20 10 30	10 26·9
Thur	27			**2nd** Tr. I. **2nd** Sh. I. **2nd** Tr. E.	9 21 11 47 12 10	10 23·0
Fri	28					10 19·1
Sat	29	13	Conjunction of Moon and Venus 1° 18′ N.	**2nd** Ec. R.	9 36 53	10 15·1
Sun	30	18	Conjunction of Neptune and Mercury 0° 32′ N.			10 11·2
Mon	31			**3rd** Tr. I.	10 42	10 7·3
JUNE Tues	1	1	Conjunction of Moon and Mercury 4° 3′ N.	**1st** Oc. D.	11 36	10 3·3

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	° ′		° ′		° ′
May 1	—68 57 E.	May 11	—11 2 M.	May 21	+46 49 E.
2	81 10	12	23 14	22	34 37
3	+86 38 M.	13	35 28	23	22 24
4	74 26	14	47 41	24	10 11
5	62 13	15	59 53	25	— 2 2
6	50 1	16	72 6	26	14 16
7	37 48	17	84 19	27	26 28
8	25 35	18	+83 28 E.	28	38 41
9	13 23	19	71 15	29	50 54
10	1 10	20	59 3	30	63 7
				31	75 21

Moon nearest the Earth, May 10, 12h.
,, farthest from ,, ,, 24, 12h.

Books received :—Liste Generale des Observatoires et des Astronomes des Société et des Revues Astronomiques. Par A. Lancaster. Bruxelles. F. Havez. 1886.—Astronomische Nachrichten.—L'Astronomie. Par M. C. Flammarion.—Bulletin Astronomique de l'Observatoire de Paris. Mars. 1886.—Sirius—Sidereal Messenger.—Traité de la Determination des Orbites de Comètes et des Planetes. Par Chevalier Théodore d'Oppoltzer. Premiere Volume. Paris; Gauthier-Villars. 1886.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 282. JUNE. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1886—87.

The May meeting was held at the Society's Rooms, Burlington House, on the evening of Friday, May the 14th, 1886.

J. W. L. Glaisher, Esq., *President*, in the Chair.

Wm. Edgar Butcher, H.M.S. "Superb," Mediterranean Squadron.
J. D. McClure, B.A., 71, Jesus Lane, Cambridge.
Chas. Ridley Smith, 44, Lower Belgrave Street, Eaton Square, S.W.,

were balloted for, and duly elected Fellows of the Society.

The minutes of the previous meeting were read and confirmed.

Col. Tupman: Eighty-two presents have been received since the last meeting. Calling for special mention are some more of the beautiful photographs of the brothers Henry, of Paris; and there is a wonderfully good engraving, made from one of their photographs, of the Pleiades group, showing the three new nebulæ and the stars down to the 16th magnitude, which probably may be considered to be the smallest magnitude that has yet been photographed. There is one large photograph of the cluster in the constellation Gemini, and a beautiful series of photographs of Jupiter.

A vote of thanks to the donors, especially to the Brothers Henry, for their photographs, was then passed.

Mr. Knobel: I should like to remark that in the photograph of Jupiter the red spot is more conspicuous than it appears to the naked eye, and also that there is a shade upon the following limb of the planet, which was nearly in quadrature at the time.

It shows the great beauty of the photographs, and is, I should think, quite unique.

Mr. Ranyard: The photographs of Saturn showed the differences of brightness between the poles of the planet and the equatorial regions more strikingly than they can be seen with the eye, and the different brightness of different parts of the ring was also more striking than to the naked eye.

Mr. Knobel: That is so.

The President: The first paper is *On the proper motions of* 29 *telescopic stars,* by Dr. Dreyer; and as he is present, I will invite him to come forward.

Dr. Dreyer said the subject of this paper is the discussion of the proper motions of 29 telescopic stars, nearly all of which are below the sixth magnitude. Of late years it has become known that proper motions of telescopic stars are by no means so scarce as used to be supposed. I believe that we now know between 600 and 700 telescopic stars, which undoubtedly have proper motions. The principal list of these stars is contained in the seventh vol. of the *Bonn Observations.* That list contains 250 stars. Then there is a second list, hitherto unpublished, also by Argelander. I quite accidentally came across the results of that second paper in a German publication, by Dr. Fisher, some two or three years ago, in which he investigates the direction of the proper motion of the solar system by means of 480 Argelander stars. During the last few years I have been almost exclusively occupied with the preparation for the press of a new catalogue founded on the observations made at Armagh since 1859; and while engaged upon that work I have come across a considerable number of stars which appear to have proper motions. First of all, I looked to see that these stars were not old friends contained in those two papers of Argelander, and next, to see whether they might have been found before by other investigators. In many cases I found that their proper motion had been already recognized; but when I could not find that that was the case, I have put the materials together for determining their proper motions, and the result of my examination is given in this paper. We are at present labouring under several disadvantages if we wish to determine the proper motions of any star. There is not only the systematic corrections for the different star catalogues, in which a good deal has been done of late years, though perhaps some of the systematic corrections are not well determined; but, secondly, what is more important, we are to a great extent thrown back on the *zone* observations of Lalande and Bessel, in which there are errors that make one cautious in using them for such a purpose. Not only have many of Bessel's places been found to have very considerable systematic errors, but in Lalande's catalogue there are many remarkable errors of one second in R.A., which probably arise from the way in which the observations were made. The observations were made by two persons: one, who sat at the telescope and observed the transit of the star over one, two, or three wires, and called out "Now," or some such word, to an assistant who noted the clock, and probably the errors of exactly one second in each case occurred owing to the way in which the assistant read the clock. I only mention this to show that some caution is necessary. Still, I have thought that my humble contribution to this important department of sidereal astronomy may have some value, if not to give the accurate values, still to draw attention to the subject, so that they may be more observed in the future. Another circumstance that may very well be mentioned is that we shall be very soon in possession of more important and valuable observations than could be obtained with the instruments in use at Armagh. The Paris Observatory has been engaged in redetermining the positions of all the stars in Lalande's catalogue, and they will be included in the great Paris catalogue; and we may expect to see an important catalogue from the Brussels Observatory, where the proper motions of stars have been particularly attended to for a number of years. These different considerations have induced me to put together these determinations of proper motions. When I have found a star occurred in more than two old catalogues, I have determined the amount of proper motion by the method of least squares; and when I have found the star only appears in Lalande's and Bessel's catalogues, I have taken the mean of the two. I will only mention that it is highly desirable that these stars should be observed from time to time, so as to determine more accurate values for their proper motions.

The President: The Society accorded a cordial welcome to Dr. Dreyer when he appeared amongst us this evening. He has spoken very modestly of this interesting paper. I will now invite comments upon it from the Fellows of the Society.

Captain Noble: I should like to ask Dr. Bischoff one question. He referred incidentally to some researches of Dr. Fisher which I have never heard of before. I should like to know whether his results bring out the same apex of the solar motion as that with which we are so familiar in the text-books?

Dr. Dreyer: Yes; as far as I remember the result was very much in accordance with what is generally adopted.

Mr. Knobel: Do I understand Dr. Dreyer to intimate that there is a paper of Argelander's not yet published containing the proper motion of several hundred telescopic stars?

Dr. Dreyer: Yes; the only time I have seen this paper mentioned is in an obituary notice of Argelander, which appeared in the *Vierteljahrschrift*, in 1875 or 1876; and at the meeting of the Astronomische Gesellschaft, at Hamburg, in 1873, I remember Argelander read a short paper on some new proper motions, and I know there was a short extract of that published in the *Vierteljahrschrift*, but that only gives the results for a small number of stars, whereas the paper which I suppose was intended to be published in the next volume of the *Bonn Observations* contained 230 new stars. It is very strange it has not yet been published. At least, I do not know that it has been published, and I have heard of no one who knows anything about it.

Mr. Knobel: Does Dr. Schönfeld possess the manuscript?

Dr. Dreyer: No doubt he does, and it will appear in one of the next volumes of the *Bonn Observations*. The result of the proper motions is given in this pamphlet.

The President: Might I ask whether the one second of error in Lalande's observations is perfectly well recognized, or whether it is the result of your own work?

Dr. Dreyer: Oh, yes, I think. Argelander calls attention to that in his well-known list of errors, which is also published in the seventh volume of the *Bonn Observations*, but I am not aware whether any one has remarked how frequently the plus sign errors occur. In a review of a former paper of mine, Prof. Schönfeld mentioned that he had gone through some cases I have mentioned in the paper, and scarcely in one case did he say he had any serious doubt as to the necessity of making this correction of one second.

Mr. Knobel: What explanation do you give of this curious error?

Dr. Dreyer: That there have been two observers engaged at the same time, one looking through the telescope and saying what he saw, and another writing it down and looking at the clock; and you can easily imagine that a person might have the peculiar habit of making an error of one second. Another thing is, in many cases in which he observed the transit he only gives the second without adding any fraction; but still you cannot be sure that those are mere rough observations, because if you compare a number of cases it is evident he has added a nought decimal or left it out at random; so that every case has to be considered on its own merits, whether the observation is accurate or not. But all the same, of course these zones are a perfect treasure to us, because Lalande and his nephew were the only people who observed these stars.

Mr. Grubb was called upon to describe, by the aid of a model, an observatory and equatorial mounting he had designed for the authorities of the Lick Observatory in California. He said: About three months ago the trustees of the Lick Observatory invited me to design an equatorial mounting and dome for the great 36-inch refractor, the glasses of which Messrs. Alvan Clark, of Boston, are now working. They mentioned at the same time that a similar invitation had been sent to three other optical firms. In the working out of that design I found it desirable to make a model, from parts of models I already possessed, and I thought it would interest the Society to inspect it. The model is not to be considered as an accurate scale model of my design, for the Lick Observatory, as it is made up of portions belonging to other models, but it is sufficient to illustrate the principle of the scheme I have endeavoured to carry out. The idea I have had all through is to bring under the direct control of the observer all the required motions of the instrument, and of the dome, so as to give him as little physical exertion as possible. The object-glass will be 36 inches in diameter, the focal length will be 57 feet; the dome will have an internal diameter of 70 feet, and 76 feet external diameter. To carry out my idea, I have arranged that there shall be four small water engines, one connected with the R.A. motion, one for the Declination motion, one for the dome, and another for the purpose of bringing the observer himself into the proper position for observing. These water engines I propose to control by an electrical apparatus, which the observer will carry about with him; but as it is undesirable to give electricity too much to do in an observatory, I do not propose that it shall be used for opening and shutting the valves of the engine directly. That operation will be effected by a mechanism which is supposed to be wound up every day by an assistant; it consists of a barrel and a few weights. This apparatus will open or close the valves, and it will itself be controlled by electricity. In this model the engines are represented by a small piece of clock-work, and the clock-work is controlled by little electro-magnets. The idea is this, that when the observer enters the room he should find at the door an apparatus of this sort. [A small case folding like a book, attached to a rope of insulated wires; it contains nine keys controlling the electric current, and is of a size convenient to be put in the pocket.] One tap on a key will cause the dome to turn in one direction, another will reverse the dome; a third key will control the telescope in R.A., and another in Declination, and so on, while there is one for lighting up the observatory, and lastly, in order that the observer may have as little difficulty as possible in getting into a position to observe, instead of getting into a chair, which perhaps would require to be 25 feet high, here is a key which will make the whole floor move up or down, so that the observer can be brought up to within a few feet of the level of the eyepiece, and can comfortably sit on a low chair without fear of falling or accident. I am sure that an observer cannot do his best work if he is perched up at a great height above the floor, or if he has to employ any exertion in moving his seat or the instrument. The machinery which would raise the floor of the Lick Observatory would be strong enough to allow a ton and a half of observers to be carried up with it. Such a force might do some damage if the wrong key were touched and the floor went up while the observer had his eye at the eyepiece. I have, therefore, thought it well to provide against such an accident by hanging a weight near to the eye-end of the telescope, which, when it touches the ground, would instantly cut off the water supply, and nothing could happen after that. (Applause.)

The President: It makes one feel quite envious to think of the luxuries to be provided for the astronomers of the future. The most charming part of the contrivance to me is the movement of the floor. The life of an astronomer with a key in his hand which he has only to touch to make all these movements seems almost ideal—something that one might dream of but could not hope to realize.

Mr. Ranyard: I suppose there will be a wall round the outside, and that there will not be open pillars as in the model. If so, how is the observer to get in when the floor is at different heights?

Mr. Grubb: Certainly. This is merely a typical model. These pillars are made for the convenience of showing it. In reality there will be a staircase in the stone wall.

Capt. Noble: We may suppose that the door moves with the floor, otherwise a gentleman might go in, say after dinner, and the floor might be down 30 or 40 feet, and he might go in at the top, and then the observation would be spoiled.

Mr. Common: I have had the pleasure of seeing this model at the Royal Institution when Mr. Grubb gave his lecture a few weeks ago, and I was very much struck with the most ingenious arrangements he has here. I rather fancied at the time that it was the actual model of what they were going to have at Lick, and I, like our President, rather envied them their luxuries. But it is not to be so, I understand.

Mr. Grubb: It is merely a design as yet, but whether they accept it remains to be seen.

Mr. Common: I had a letter from Professor Holden the other day which may be of some interest to this Society. I will read parts of it with reference to what they are going to have. Professor Holden says: "The glass for this lens"—in referring to the object-glass—"is now available, and we hope during the early part of 1887 that we may see the objective completely finished, and perhaps delivered in California. Our large dome will undoubtedly be finished during the current year, and we look forward to commencing serious work with the whole Observatory during the year 1887, and possibly sooner." That rather shook my faith in what I had previously conceived to be a settled thing—that this was going to be set up in California; and I saw in last month's number of the *Century Magazine* an idea which seems to have been projected by a young engineer who was employed, and has carried out, most of the work at Mount Hamilton; he proposes to erect a dome, which he calls a seven-eighths dome, that is, a spherical building, or seven-eighths of a sphere, erected round the instrument. The upper hemisphere would correspond to the ordinary observatory dome with its shutters, and the lower three-eighths of the sphere would be furnished on the inside with tiers of steps, so that the observer could always get up to the eye end of the telescope. [Mr. Common then drew on the blackboard what he believed to be the pattern of the dome, and also described a method he had communicated to Professor Holden for a mercurial floating of the dome.] Apart from that I must say that everything that contributes to the comfort, or to the ease, of the observer, or to the despatch of business while he is observing, is of the utmost importance. If you have to lose valuable time in doing absolutely necessary operations, even moving about and shifting the chair, or doing this and that with the dome or instrument, you lose time you could otherwise usefully employ; and any care or trouble taken in the preparation of an instrument to enable a man, when fine weather comes, to use the telescope to its utmost capacity, is a distinct gain. It increases not only the comfort of the observer, but the value of the instrument; because, if you have a large instrument only working half time, it is of half the value it would be if it was working full time. Professor Holden says: "It is my hope that we may be able to keep the telescope at work during the larger part of the 24 hours, day and night." In all these cases it is not fair to judge by models, or by one solitary attempt. When things are new they do not, perhaps, fall in as the inventor intended, but the system may be perfectly right, and it is only through lack of some minor detail that it may fall into bad odour. If the principle is right of saving as much labour as you can to the observer, the result will naturally follow that you get at last the perfection of mechanism that is absolutely needed.

Mr. Stone: May I ask if there is not a certain amount of danger in using water-power? On certain occasions water freezes.

Mr. Common: Put a little magnesia or soda in it.

Mr. Stone: Of course all these ideas may be carried out with other motive power as well as water. So far as water is concerned there is a danger in the English climate. At times in winter when the sky is exceedingly clear, and when one might want to make observations, it may be that your water-power is cut off. I do not think water-power, if you want to keep the instrument incessantly employed, is altogether a safe motive power. With regard to the idea Mr. Grubb wishes to carry out, it makes one wish to be able to go over life again to have such an instrument to use. Any one who has employed an instrument of any size, and has moved it about, and moved the dome about, and wheeled the chair about in the ordinary way, and made observations in the ordinary way, knows that at the end of a long night one feels excessively knocked-up, and knows that he could have done a good deal more had such contrivances been in his hands.

Mr. Bryant: To prevent the water freezing could not it be impregnated with chloride of magnesia? I believe that this plan has been adopted at the Nice Observatory, and with very satisfactory results.

Mr. Grubb: With regard to Mr. Stone's question, I have made inquiry. I had the choice of steam, gas, or water, as a motive power. Steam I thought to be rather impracticable; as to gas, I was not aware that they had gas there. I then inquired with regard to the matter of freezing, and I was informed that they had hardly any frost whatever there, as the winter was very mild. As Mr. Stone says, any other motive power could be applied in the same way. As to Mr. Common's remarks, he has touched upon the contract and the time, so perhaps it might be interesting if I mentioned to you that it is about three months ago since I was first applied to. I was told that the estimates for the equatorial should be in San Francisco on the 1st of May. Subsequently they applied for an estimate for the dome. Of course in sending in an estimate for the equatorial I was obliged to include the dome as a portion of the system; but afterwards they sent for an estimate for the dome, and said that the estimate should be in San Francisco by the 15th of May, and that the dome should be complete and in working order in the month of October this year. It would be impossible to get matters arranged until 1st of July at the earliest date, and allowing a month for shipment, and a month for erection, it will leave me one month to make it, and there will be £4,000 to spend upon the dome. I had to inform them that they would have to turn months into years before I could do this kind of thing. That is as far as I know about this state of things, so that it rather tallies with what Mr. Common says about expecting to have everything ready this year.

The President: Whether the actual construction of this dome for the Lick Observatory falls to Mr. Grubb or not, I am sure we are equally indebted to him for bringing before us this beautiful model. At all events it represents exactly what the astronomer needs—that he should be able to hold in his hand a handle, and be able to press down different buttons on that handle, and so move the telescope or the floor just as he pleases. That is what we certainly shall come to in the future. This model marks a step in the progress of astronomy, and it, or something like it, will probably be adopted sooner or later in all large observatories, so that the astronomer will be able to devote his whole energy to observing what he sees through the telescope without having his time and thoughts occupied in what can be done so much more easily by the methods Mr. Grubb has explained.

Dr. Leonard Waldo, of Yale College, was called upon to describe an escapement for clocks of precision. He said:—I am sorry, but a sick child of a countryman of mine claimed my attention up to the time of coming to the meeting, or I should have brought with me an escapement which represents the progress we have made on our side of the water in the study of precision clocks. It will be a little more interesting perhaps if I review slightly the experiments which have been made, largely under my direction, having in view an attempt to measure the hour with the same proportionate precision with which we can measure the day. The ordinary clock theory assumes that our pendulum swings in a plane; that it has a given arc of vibration, and the variation in the arc is compensated by change in the pendulum; and a good many other assumptions which will not bear close investigation. Perhaps there has been of late years more attention than usual devoted to standards of all kinds on the other side of the water, and having the facilities of some of our large clock companies—and the facilities of the largest were very good indeed—it rather fell to me to undertake an examination of clocks of precision. I introduce that term because the ordinary clock of the observatory is, in general, more or less exposed, and we all know how barometric and thermometric changes introduce gross errors in clocks, and there are more or less obscure errors which may be called differentials of a higher order with reference to ordinary errors, these differentials of a higher order depend upon such obscure causes as want of vibration in the plane, mobility of the support, change of the co-efficient of the elasticity of the suspension spring, change of temperature, change in the arc of vibration, want of conformity in the arc to a truly circular or cycloidal arc, as the case may be. It is very clear indeed that when we compensate the ordinary steel spring suspension pendulum, we compensate the change in the spring itself which takes place in a change of temperature, and that quite as much as the change in the length of the pendulum. A great deal of attention has been paid to the actual change in the lineal diminution of the rod corresponding to the change of the steel or glass bar at the base, while little attention has been given to change in elasticity and other conditions of suspension, but these are important considerations. Therefore, the first question was to see whether the pendulum could not be swung in some other way. I did not quite see the way to swing it in any other way, but Sir William Thompson said he would like to show me a clock in which he had carried out an idea on which I had worked. He said he had swung a pendulum on an agate knife edge circular at the top. Yesterday I saw it and roughly it is this. [Blackboard.] I had thought out the idea of having a suspension analogous to that—and the two suspensions are identical. I had also been experimenting with glass rods, the co-efficient of expansion of glass being much less than steel or platinum; and the temptation is great to use glass rods in pendulums, but I have not been able to get a glass bar which satisfies me. Sir William Thompson showed me his pendulum with an agate suspension and a glass rod and glass bob at the bottom, and mechanically, the difficulties have been overcome with one exception, viz.: that I do not see that he had any guarantee that the inside of the glass bob was a true cylinder. In any glass-blowing the imperfections of the cylinder are of the utmost consequence, because no compensation can take place worthy of the name, in a precision clock unless the jar is a true cylinder. I rejected glass because I did not see the way to get a cylinder or homogeneity of the rod. All the efforts I made to get absolutely straight rods were failures. We may succeed; but at present we have gone back to steel jars with steel rods, but with this difference :—You will remember in the theory of clocks that all the errors of escapement are expressed by a fraction, in which the sum of a certain number of errors is divided by the constant a, multiplied by the weight and length of the pendulum itself. There are two reasons why the length of the pendulum cannot be very well increased; one is, the unit second limit of time, and the other, that no compensation yet provided has given the actual results which mercurial compensation permits. There is an uncertainty about a zinc and steel rod totally unexplained, and yet the gridiron pendulum has reached its highest development in the gridiron pendulum made by *Kessels:* but all the clocks of modern times, the Hohwü clocks and those made by *Tiede*:—those which show the best performances—are clocks where mercurial compensation has been used, and, I take it, it is owing to that extreme sensibility to change where a liquid flows instead of a solid expands. The fractional expression shows us the necessity of increasing the weight of the pendulum; and therefore, the pendulums in my experiments are very heavy—from 45 to 60 lbs. of mercury—for the ordinary mercurial pendulum. There are two other things ordinarily in the pendulum jar, beside the mercury:—bubbles of air and oxide of mercury. The centre of gravity, by the solution of the oxide of mercury changes; and therefore you must have pure mercury free from air; and this is ordinarily obtained by distilling the mercury in glass so as to be free from the oxide. So much for the pendulum. The next consideration, is the restoring the loss of motion of the pendulum by the escapement. Now, while in an ordinary counting clock the best escapement is, perhaps, a Graham escapement, yet when you put it 1,000 feet from the observer, using it not only as a standard of time, but for doing some other work of the observatory, the Graham escapement will not do, because any additional work, electrical or otherwise, directly affects the escapement itself. On that account, I think some form of detached gravity escapement is the escapement of the future. The point of application of that escapement is an important consideration. In *Breguet's* escapement the point of application is at the base of the bob: but I prefer the established type for applying impulse at the ordinary point. I began to experiment with an escapement analogous to the Dennison (gravity) which is quite a favourite in the United States, because Messrs. Howard have made some of the best clocks with that escapement. But Denison's escapement required a large amount of power to drive it, and I have never seen a Denison gravity escapement which did not stop within two years; and that is a serious interruption where you are conducting observations. I took it that the great difficulty with Denison's escapement lay in the fact that the machine is a train of wheels, which should start at the end of every second from a state of absolute rest, at a certain velocity, and throw its arms out in time to be locked before the second closed, but somehow or other it fails to get up sufficient velocity, unless the train of wheels is allowed to sustain a weight utterly out of the question. Therefore, I adopted a very simple device suggested by Professor Lyman, one of those men who unite theoretical and mechanical science in a very high degree. He suggested putting a large friction roller in one of the arms of Denison's escapement, and to lift that arm by an equilateral triangle, which in a position of rest should be by the side of the friction roller. When the arm is lifted it is by the rotation of this triangle on the axis. When the clock begins to lift it will be with a very small velocity; but as the triangle rotates on its axis it will be with an increasing velocity until the arm is lifted up, having its minimum velocity just as it stops. By this means you can drive a heavy pendulum with the driving weight reduced to a minimum, and the certainty of action increased to a maximum. If you take care of the pendulum and the escapement, then comes the very important question of providing a rigid support for the pendulum. I have adopted two piers of heavy masonry work similar to the piers of a transit instrument, with a massive iron arch on the top, from one pier to the other, and from this arch, bolted down, the pendulum can swing; and then you can put the escapement on such a bracket as you please. Then, I have a little vernier attached to the pendulum swinging across a scale at the bottom. Built inside the case surrounding each pier are coils of steam-piping for regulating the temperature, and a thermometer made of a layer of zinc, or steel, and rubber. These are the salient features of the experiments which I have been carrying out for a couple of years back; I can only say we have passed the point of present attainment of certainty in clock rates of precision. Perhaps, the total amount of average deviation will be lessened by 25 per cent. That I regard as the probable estimate of the value of such precautions in the clocks built on this principle, and in the best forms of time-pieces. We now have that great advantage that you can by means of auxiliary relays outside the clock transfer the precision clock, which may be in a cellar or well, to any part of the observatory. As to the immediate differentiation of this species of clock with auxiliary pendulum, we now load up the impulse-giving apparatus of the pendulum, and we move the hands and drive the train, we send electric signals all round the observatory: but that is wrong. There ought to be an auxiliary in the clock driven by another pendulum, say a half-seconds one for doing that special piece of work. That idea was carried out by Richard Bond, the brother of W. C. Bond of Harvard College, by means mostly of his gravity escapement. There is a clock of that kind in the Liverpool Observatory, and I have seen another at Sir W. Thompson's. He has the auxiliary work done by an auxiliary pendulum, as it should be; and undoubtedly that will be the next step of progress in the construction of such clocks of precision.

The President: We are very much obliged for the interesting account given by Dr. Waldo of his researches on the subject of clocks. (Hear, hear.)

Mr. Buckney: I should like to make a few remarks on what Dr. Waldo has said, because his experience disagrees with mine. First with regard to the pendulum. I take it he considers the mercurial pendulum is far better than the zinc and steel one. My experience has shown quite the opposite. I very much prefer the zinc and steel pendulum. I found some years ago, in making experiments, a great many errors in the mercurial pendulum, at any rate one particular error it seems to possess, and the more you increase the size of the pendulum the more you aggravate that error. With the mercurial pendulum, any change of temperature will act first of all on the vessel containing the mercury, and will change its dimensions. If you expose an iron vessel containing mercury to a sudden change of temperature, such as putting it suddenly into warm water, the first effect of that increase of temperature is to make the mercury fall in the vessel, that is due to the sudden change of temperature, causing the cylinder or jar to expand before the temperature has time to act on the mercury. The effect is transient. The mercury soon recovers itself, and takes up the new temperature, but some time elapses, and of course during that time the clock is losing. I also made some experiments with regard to the advantages of different sizes of mercurial pendulums, and I found that a mercurial jar of two inches in diameter, which is the ordinary size used when exposed to a change of temperature amounting to 30°, took about one day to adjust itself. With a change of temperature of 30°, a jar three inches in diameter took two days, and sometimes three, to adjust its rate. The cooling was effected in about half the time. The increase of temperature took about double the time to show itself that the cooling effect did. That I cannot account for, but I mentioned it to Mr. Glaisher, the father of our President, who was then at the Royal Observatory, and he mentioned that he had noticed the same difference with thermometers. Then I found that the compensating effect took very much longer in a mercurial pendulum than in a zinc and steel pendulum, and that arose from the mercury being in large masses, and the change of temperature took some time to produce an effect upon the mass in the bob. With the zinc and steel, however, you have simply a steel rod going down, generally a tube of zinc outside it, with another tube of steel, and the bob is suspended from that at the centre, By suspending the bob at the centre, you eliminate any errors owing to the expansion of the bob itself. You depend for the compensation effect upon the zinc and steel forming the rod of the pendulum. Now, these metal rods are made pretty much of the same mass, and any change of temperature affects them almost simultaneously, which is a very great point. But in the mercurial pendulum you have the rod itself comparatively small, and no doubt a change of temperature causes the rod to lengthen some considerable time before the mercury takes up the new temperature and expands to produce the compensating effect. As to the escapement, the detached or Graham escapements, it appears to me, have in the past answered exceedingly well, and with regard to electric-contact apparatus applied to an escapement of that kind, the only point essential is that the impulse should be completed before the electric contact is made, so that the impulse given to the clock should not be interfered with or affected in any way by the act of making the contact with the springs. With regard to the suspension of the pendulum, I think it is admitted on all sides that it cannot be too rigid. I think it may be suspended in another way than shown by Dr. Waldo. I have generally adopted the plan of having a massive bracket bolted to metal blocks let into the wall; fastened with strong bolts you get a most rigid suspension. I do not think that it much matters how the suspension is made so that the point of suspension is rigid.

Mr. Inwards: It seems to me there is another objection to the mercurial pendulum, though a small one, and that is that the mercury must, to a certain extent, flop about in the pendulum, and that must be equivalent to so much friction.

Mr. Common: There is one point I should like to speak about. With regard to the gravity escapement, I have always noticed that its arm, upon which the power to keep the pendulum going depends, always swings from an ordinary arber which is oiled in the usual way. When we consider the effect of the slightest stickage in the oil it must make an enormous difference to the amount of the impulse given to the pendulum. It has always struck me that if clock-makers of precision will do away with this arber, and substitute a spring suspension, instead of the ordinary arber mechanical suspension, they would get rid of a great deal of difficulty; I do not know whether that is a point Dr. Waldo has taken a note of. There is another point about the oscillation of the pendulum. Any unsymmetrical pendulum has always a tendency to wobble without any regard to the steadiness of the support. There is also a constant tendency of the pendulum to turn round, when once set swinging, with the motion of the earth, and alter its plane, and there is a constant struggle to change that plane of oscillation. It is like keeping the spring bent in a permanent manner, and always subject to a slightly varying weight. With regard to the testing of the steadiness of the point of oscillation there is a piece of apparatus, which was devised by Admiral Smythe, to test the steadiness—we have one in our museum—he called the instrument a "noddy." It is a delicately suspended ball, that will take up any slight vibration that existed at the point of suspension, and so render it visible after a very short time. There is one point on which both Mr. Buckney and Dr. Waldo were very reticent, and on which I feel sure a good many practical astronomers would be glad of a little information. They all talk of making contact in working clocks, but the question is, How are those contacts made? Every man who has tried to make them, so far as I know, has found a considerable difficulty, and no doubt we should be glad if some really practical plan could be invented by which an ordinary gravity clock, or dead-beat escapement, could be applied for the purpose of using either the chronograph or a secondary pendulum.

Dr. Waldo: This criticism is very interesting indeed. I do not know whether I can remember all the points brought out, but they all claimed a good deal of attention. In the first place, about the zinc and steel compensation, I am sorry to take up the cudgels on zinc and steel *versus* mercurial compensation, but I should like to say if any member or Fellow can point out to me a case in which a zinc or steel compensation has been thoroughly investigated, and can show me papers, or transactions, or figures, on which the excellence of that actual work depends, I shall be very much obliged to him, for I have sought far and wide for just that kind of literature. I know the first thing you will say is, "Go to the Greenwich Observatory, and you will get a complete investigation of the pendulum," but unfortunately you do not get anything of the kind, and when you go you find the clocks have been running only for short intervals without disturbance. I can assure you that accurate observations of clock rates under conditions with all the quantities measured are exceedingly rare and difficult to get, and of all the observations made I think you will find that the mercury-steel pendulum gives the best results on which you can work. But I have a second point. General Comstock, of the U.S. Lake Survey, pointed out very clearly that for standards of length, zinc cannot be employed. It has a certain co-efficient of expansion, and after it has cooled down again, it never takes up the same position. The "swish" of the pendulum with mercury bob, I regard as a valuable thing to point out, because it has given a great deal of trouble, at least theoretically. I regard the idea of having smaller diameters and parallel cylinders of mercury as a bad one, because of the difficulty of having in these matters the metal expand in a uniform direction, and having cylinders parallel to themselves. Therefore I prefer to either put up with the evil, or else have the arc of vibration extremely small. I frankly acknowledge that is a difficulty, and one which has not been surmounted except by the clumsy expedient of having a number of parallel cylinders, and then the precision of the instrument is not sufficiently great to be sure that this acts quite uniformly. Another point was the suspension of this gravity arm. I have made this pivot run in a dry jewel hole, but perhaps it is better to have it with spring or friction rollers. The elder Bond, who built a most beautiful specimen of workmanship in his gravity escapement, had springs. In designing these clocks, I queried a long time whether to put the springs in there or let the arms swing on friction rollers. The point brought up about making contact in the clocks, I heartily sympathize with the gentleman making that remark, because of all difficulties a man meets with in observatory work, a chronographic system of registering is a serious difficulty. I can only say how I make contact in this clock, and what means I use outside the clock. I do not say it is the best, I only say it gives very satisfactory results. In the first place contact fails to work, because the oxide in the metal that forms the support leaves some dirt. If you use a platinum and gold point in making contact, you will get a little less spark than with almost any other metal. That is merely a shop dodge. I do not know why it should be so. A clock I have made in that way has only been running eight or nine months, so next year I may have all the trouble again. I merely give that as a present solution of the very difficult little problem of how to make this contact inside a clock. If you want to get rid of the spark, it is a simple device to put in a shunting coil of German silver wire with a constant battery, and outside the actual contact you can get at and clean the point.

The President: The meeting have already thanked Dr. Waldo cordially for his lecture. I can only say that this is the first time I have heard any discussion on the relative merits of mercurial and gridiron pendulums, and I have listened to it with the greatest possible interest. I have often wondered that the merits of these different pendulums have not been discussed, as far as I know, in any book.

Mr. Green read a paper in reply to some critical remarks published by Mr. Knobel on the markings of Mars, the northern hemisphere of. Mr. Green illustrated his remarks by drawings on the black-board. The existence of Huggins' Inlet he felt sure of; having made several drawings of it but broader and more evident than as shown in Mr. Knobel's drawings. He had made a drawing on February 12, 1884, in which the termination of Nasmyth's Inlet answers exactly to Mr. Knobel's description; but during this opposition the old mark Lassell Sea has reappeared with greater distinctness than before. He had seven drawings all confirming the shape of it he had given. It was a marking that could be seen by strangers on looking at the planet for the first time.

The President said that he regretted Mr. Knobel had not been able to remain to hear this paper, he had been obliged to leave to catch a train.

The following papers were also announced:—

G. W. Hough: *Observations of the Companion of Sirius, made at the Dearborn Observatory, Chicago.*

W. F. Denning: *Jupiter's third Satellite in transit, April* 11, 1886.

J. E. Gore: *Observations of the New Star in Orion.*

W. F. Denning: *The Meteor Shower of Halley's Comet.*

B. J. Hopkins: *Note on a remarkable Sun Spot.*

Rev. T. E. Espin: *Photographic Photometry, Zone* + 40°, oh. — 1h.

Royal Observatory, Greenwich: *Observations of Comets* d, 1885 (*Fabry*); e, 1885 (*Barnard*); *and* a, 1886 (*Brooks*).

E. J. Spitta: *The IVth Satellite of Jupiter during Superior Conjunction, on the night of April* 5, 1886.

A. M. W. Downing: *Comparisons of certain Southern Star Catalogues.*

The meeting adjourned at ten o'clock.

LIVERPOOL ASTRONOMICAL SOCIETY.

A statement was read by the President at the last meeting, and has been reported *verbatim* in the *English Mechanic.* It is reasonable to suppose that a similar report will be sent to the *Register.* Now there are several inaccuracies in the printed report which I had pointed out at the meeting, but as my corrections do not appear, and the errors are still persisted in, I will ask you to allow me the space to repeat them.

First. It is uncharitable to assume that the most active promoters of the society relinquished their share in the work "when the novelty of their position settled down into routine." As a matter of fact, most arduous routine work was continued by them for four years without a break, and, after one year's relaxation, their names are again on the list of active workers.

Second. The annual subscription of 5s. has hitherto proved sufficient for the working expenses. The session of 1884-85, which Mr. Roberts is so hard on, was an exceptionally heavy one. I have taken some trouble to go into the accounts, and find that, although 193 pages of printed matter, besides 42 illustrations, had been sent post free to each member, the total amount of debts incurred did not exceed the income by more than £8. Even this small deficiency might have been avoided if the Directors had not provided 160 bound volumes of the journal for future contingencies.

Third. The annual meeting is not in October, but in July. At the last annual meeting it was proposed to defer it until October; but as it was shown that this would, on many accounts, be inconvenient, the motion was lost.

Fourth. It has never been found necessary that active workers should reside in Liverpool. The great object of the society, and of the publications which are the life of the society, is to record the astronomical observations of its members, and Liverpool is perhaps least of all suitable for such work. The necessity for paid assistance is negatived by the fact that such men as Denning, Franks, Espin, Elger, and others, have cheerfully volunteered regular literary and scientific work. Twice the present income would not be sufficient to pay for competent management, and, if the enthusiasm and devotion of the members should fail, mercenary assistance would avail but little.

Finally. It is a pity that Mr. Roberts should have troubled himself to prepare a report on the affairs of the society at all. It is distinctly stated in bye-law 4 that this shall be the work of the Council, and that the report shall be presented at the annual meeting. The object of this law is, plainly, to frustrate any attempt at influencing the general election of officers, and the closing part of Mr. Roberts' address shows only too clearly the necessity of such a provision.

W. H. DAVIES, Vice-President L.A.S.

SELENOGRAPHICAL NOTES, JUNE, 1886.

By THOS. GWYN ELGER, F.R.A.S.

Stadius at sunrise and at mid-day. In Schmidt's large lunar chart Stadius is drawn open towards the north without the faintest indication of a border. Mädler merely shadows forth the completeness of the ring by delicate shading, and shows nothing in the shape of a wall as defining the limits of the formation. The following observations relating to the northern side of the ring-plain may be of interest. They were made between 7h. 30m. and 9h. 30m. on April 12, at which time the great crater-row west of Copernicus was close to the morning terminator, and the broad shadow thrown by the lofty mountain-arm connecting Eratosthenes and Stadius extended beyond, and partially obscured two of the principal component craters of the row. West of the brilliant crater B and its associated group of minute craterlets, there is a large cup-like depression only visible under very oblique light. These objects stand at a higher level than that of the neighbouring interior, as shown by a narrow, but well marked, band of shadow which defines the north-west limits of the ring-plain, not due to any distinct form of barrier or wall, but to a drop in the surface of the region from the outside of Stadius to the floor. A little south-east of B lies another bright but smaller crater, also surrounded by minuter objects of the same class. From this to a group of large craters near the south end of the Copernicus crater-row, extends a very narrow strip of wall, casting, even when close to the terminator, only a thread-like line of shadow. At times of best definition it resembled a ridge of a very attenuated type, or a delicate light streak bordered on one side by a fine dark edge. This is all that represents the north-east border of Stadius under the best possible conditions for observing it. A number of good-sized crater-cones are visible at sunrise on the floor, but more are revealed at a later stage of illumination. The majority of them are larger than most of the similar features found on the floors of Archimedes and Plato, and are presumably more plentifully distributed, as upwards of 30 have been recorded since November, 1885, within a comparatively limited area.

Though it is not strictly true that a high sun effaces either formation, Mädler's dictum "that the full moon knows no Maginus," might be applied with equal propriety to Stadius. A search made under these conditions, say, when the moon is within a day or two of full, is not an easy matter. Unless we are familiar with the illuminated aspect of a few landmarks in the vicinity, such as Eratosthenes and the region round Copernicus, we may sweep in vain for a long time ere we find Stadius. If, however, the crater B, which, owing to its glittering border, is always a brilliant object, can be detected, we shall discover that more can be made out of Stadius under a high sun than might be supposed. On April 16th, when the moon was within two days of full, I was able to see distinctly four of the most southerly of the crates composing the crater-row of Copernicus, and faint traces of the remainder. The interiors of the former appear to be covered with some substance of inferior reflecting-power, so that even when they are fully illuminated the contrast between the inside floor and the surrounding rim is sufficiently pronounced to render them easily visible; while, as regards the latter, although they are fully as large and deep, the distinction is not so apparent. Five of the principal craters on the eastern border of Stadius were also distinguished by reason of their dusky interiors. B was a brilliant object, as was also the smaller crater south-east of it; and two members of the group of craterlets associated with B were also glimpsed. On the southern quarter of the interior the little crater-row running up to the southern border was easily made out, the orifices of the component craters being observed without difficulty; but, with the exception of a few nebulous light spots, representing crater-cones, no other objects were visible on the floor, on account of the intricacy of the network of light and dark streaks traversing it. The border of Stadius was just traceable; the arm from Eratosthenes extending to the west side of the ring-plain, was a bright though ill-defined object. The south-west wall, at no time very conspicuous, was indicated by a curved dark streak. The southern and eastern limits of the formation are rendered manifest by the suggestive fact that the rays, whether silvery or dusky, emanating from the Copernicus region, become suddenly modified both in character and intensity on encountering the border of Stadius. This is a matter which involves more than one problem relating to lunar physics worthy of consideration.

Kempston, Beds: 18th May, 1886.

SIDEREAL NOTES FOR JUNE, 1886.

By J. E. Gore, F.R.A.S.

The following known variable stars may be well observed in June, and observations of them are very desirable:—

1. 10 Sagittæ. R.A. 19h. 50m. 34s., N. 16° 19′ (1880). A short period variable, discovered by the present writer in 1885. The variation is from about 5·6 to 6·4 mag. in the scale of the *Durchmusterung,* and the period about 8½ days. The increase of light is quicker than the decrease. Further observations of this variable are much to be desired, to enable its period to be determined more exactly.

2. D.M. 27°, 3890 Vulpeculæ. R.A. 20h. 46m. 24s., N.

27° 48′ (1880). A very interesting short-period variable, discovered by the eminent observer Sawyer (U.S.A.) in 1885. The variation is from 5·5 to 6·7, and the period about 4·437 days. Observations of this variable are also most desirable. It may be easily found with an opera-glass, as it lies about 45′ *preceding*, and a little north of the naked eye star 32 Vulpeculæ.

3. Birmingham, 587 Cygni = D.M. 44°, 3877. R.A. 21h. 31m. 30s., N. 44° 50′ (1880). A variable discovered by the present writer in 1884. The variation is from about 5·8 to 7·5, and the period about 120 ± days. The star was near a minimum on February 14, 1886, and near a maximum on April 30, 1886; and observations are very desirable. It may be observed all the year, as it never descends below the English horizon. A minimum will be due about the middle of June. It may be easily found with an opera-glass, as it lies closely *south following* the 4th magnitude star ρ Cygni. The spectrum is a fine example of Secchi's third type.

The following suspected variable stars may be especially recommended to the attention of observers:—

1. 83 Ursæ Majoris. R.A. 13h. 36m. 11s., N. 55° 17′ (1880). Rated 5–6 by Heis. Measured 4·83 with the photometer at Harvard, and 5·10 at Oxford. Seen by Birmingham as bright as δ Ursæ Majoris, or 3·4 mag., August 6, 1868. Birmingham says: "It was smaller on the next night, and slowly diminished to its usual magnitude." It is of a fine orange colour, and has a spectrum of Secchi's third type.

2. Lalande 26200 Virginis. R.A. 14h. 13m. 33s., N. 0° 56′ (1880). Rated 6 mag. by Heis; 5·9 at Albany, by Gould; and 6·5 and 6·6 at Cordoba. It was measured 6·11 at Harvard, and 6·31 at Oxford. On April 7, 1884, I estimated it 6·2; on April 8, 1886, 6·4; and April 21, 1886, 6·5. This star is evidently variable to a small extent.

3. Lalande 26980 Boötis. R.A. 14h. 42m. 56s., N. 6° 28′ (1880). Rated 6 mag. by Lalande, 7·5 in Durchmusterung, 6 Argelander, 6–7 Heis, and 6·8 at Cordoba. It was measured 6·61 at Harvard. On April 21, 1886, I estimated it 7·3 in a clear moonless sky.

4. Lalande 27017 Boötis. R.A. 14h. 43m. 49s., N. 8° 29′ (1880). About 2° north of the preceding, and a little *following* 31 Boötis. It was rated 7 mag. by Lalande, 7½ by Lamont, but 6 mag. by Argelander and Heis; 7·0, Gould, who considers it probably variable. It was measured 7·02 at Harvard (6·5 "estimated"), and 6·95 at Oxford (1884, 4·64). On April 20, 1884, I estimated it 6·8 in Gould's scale; on April 8, 1886, 7·4; and April 21, 1886, about 7·5. The star is evidently variable to some extent, and deserves careful observation.

5. P. XIX. 276 Cygni. R.A. 19h. 41m. 15s., N. 35° 48′ (1880). A double star ε 2578, 6·6, 7·4 : 126°·5 : 15″. Dembowski thought the components variable, and rated them 8, 8.5. They are rated 8 mag. by Lalande. The star is not in Heis' Catalogue. My own observations from September, 1885, to April, 1886, show fluctuations of light from 5·8 to 6·2 mag.

THE LATE MR. G. J. WALKER.

It is with great regret that we have to record the death of Mr. G. J. Walker, which happened at Teignmouth, Devon, on April the 23rd. We have lost in him one of our oldest subscribers, a very valued friend and fellow-helper.

Mr. Walker was born in 1812, and by the interest of his uncle, Sir George Walker, he was at an early age gazetted to H.M. 13th Light Dragoons, and served in India, having previously devoted his attention to mathematical studies under his father at Florence, and afterwards at Sandhurst. In 1837 he married the daughter of Dr. Corrie, the first Bishop of Madras, and soon afterwards left the army and retired to Teignmouth, where he devoted his time to classical and oriental studies and religious work. He was a practical and learned astronomer, taking the keenest interest in everything relating to the subject to the time of his death.

MINOR PLANETS.

It appears from observations made with the 9·6 inch equatorial at the United States Naval Observatory, Washington, that the places of the following minor planets in their respective ephemerides require to be corrected as under :—

Planet (56), mean of observations with three comparison stars, —2·5 sec., + 0″·4.

Planet (59), mean of observations with three comparison stars, + 0·19 sec., —0″·1.

Planet (62), mean of observations with the same comparison star, —0·075 sec., + 11″·55.

Planet (71), —6·07 sec., —41″·4.

Planet (185), —1·81 sec., + 13″·4.

Planet (186), mean of observations with three comparison stars, —4 sec., + 1′·3.

Planet (209), mean of observations with two comparison stars, —0·85 sec., + 18″·3.
Planet (221), + 9·53 sec., + 55″·7.
Planet (241), +81·82 sec., — 205″·4.

The observations were made at various times, ranging from March 4, 1885, to December 2, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.
To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

HEIGHT OF A LUMINOUS METEOR.

During the brilliant aurora of March 30, I saw three fine meteors. One as bright as Sirius, and having a bright sparkling train, 5° long, observed at 9.58 G.M.T., was directed from R.A. 105°, Dec. +33°, and disappeared at R.A. 62°, Dec. +24°, after a course of about 8°. This was also seen from near Darlington, by E. B. Backhouse, and though the point of appearance and direction were but roughly noted, the data appear sufficiently exact for the height of the meteor to be calculated with no great error, the resulting figures for point of appearance being 63 miles above the earth's surface. By combining my own observation of the point of disappearance with the extremely rough determined radiant at R.A. 119°, Dec. +33°, from the separately observed courses, the height at which the meteor disappeared would be 46½ miles.

The other two bright meteors, both equal to Sirius in lustre, occurred at 10.13 and 10.20, but did not proceed from the above mentioned radiant.

Sunderland: 1886, May 17. T. W. BACKHOUSE.

STAR MAGNITUDES—NOVA ANDROMEDÆ—RED STARS.

Sir,—Mr. Gore's letter in your issue for March, is very interesting. He has, however, omitted to notice two points, which may have an important relation to the cause of the differences in magnitude-estimates which he notes. In every case the *sign* of the difference is the same—*plus.* This certainly seems to point to some source of constant difference between the two catalogues. It would be interesting to know whether the same sign predominates throughout the whole series of differences, or whether its predominance is confined to the extreme cases. In the second place, all the stars included in this list are more or less orange, yellow, or at the very least tinged with either hue. ε Cephei is a fine golden star; in 8 Monocerotis the hue of the principal star is very fine gold; 1 Lacertæ, I find I have entered as "orange;" 5 Ursæ Minoris, is a fine golden yellow; and ρ Persei is a very fine orange; Webb draws attention to the "curious colour" of λ Herculis, "deep, dull, orange." Has this generally yellow or orange colour of these stars anything to do with the circumstance drawn attention to above? That the Oxford photometer, at least, is not ideally perfect, I think most readers of the notice of the "Uranometria Nova Oxoniensis," which has appeared in your columns, will allow, when they find that Sirius is, so to speak, degraded from its surpassing pre-eminence in matter of brightness. The naked eye will probably, in that instance, be regarded as giving a better account of the truth than any photometer which gives the result referred to.

Mr. Hopkins' letter on Nova Andromedæ, in the same issue of the *Astronomical Register* as Mr. Gore's, contains two points worthy of notice. In the first place, Mr. Hopkins seems to have fallen into a mistake, into which others have fallen, when he speaks of "its sudden apparition, together with its complete and rapid extinction." In no sense was the apparition of Nova Andromedæ so rapid as the apparition of some other Novas on record—the "Blage" star, or Schmidt's in Eygmas, for example. For the star was seen as of 9½ mag. by Mr. Ward on August 19, and did not attain its maximum brightness till about September 2, when it did not *exceed* the 6th magnitude. It, therefore, took about a fortnight to increase these 3 magnitudes or so—not a rapidity of increase to the maximum in anywise comparable to that shown by the stars I have mentioned, or of the star of 1572. And this is true whatever may have been the rapidity of its increase previous to August 19; though it may be observed, in connection with the reported dates upon which the star was *not* there, that negative evidence in regard to non-observation of totally unexpected phenomena cannot go for much. The second point I would like to draw attention to is Mr. Hopkins' assertion that it "would also differ from such of those bodies [known variable stars] with which we are acquainted in that it changed colour as it decreased in magnitude." Would it do so? Is it not *actually* the case that any star grows *redder* in decreasing in magnitude? This is a point which has been controverted, but I thought it had been admitted that it was at least possible; and that, moreover, it was considered that some stars which are simply *yellow* at maximum, become *red* at minimum. Of course, the change in colour in Nova Andromedæ seems to have been in the other direction—from a colour of less to one of more refrangibility, instead of from one of more to one of less—but Mr. Hopkins' is a general statement. I am not, of course, arguing the question as to whether the Nova was a true "new star," or an ordinary "variable" one, but am simply discussing the facts.

May I ask if any of your correspondents have recently obtained any observations of B 118, B 169, and B 447?

I am, Sir, yours truly,
S. MAITLAND BAIRD GEMMILL,
Glasgow: May 17, 1886.

DUN ECHT CIRCULAR, No. 114.

Professor Krueger announces from Kiel the discovery of a comet by Brooks:

1886.	Greenwich M. T.	R. A.	Decl.
	h. m.	h. m. s.	° ′
April 27	12 1·4	0 14 56	N. 61 59

Motion slow south-east.

L. BECKER.

Lord Crawford's Observatory,
Dun Echt: 1886, April 29.

DUN ECHT CIRCULAR, No. 115.

Professor Krueger telegraphs from Kiel the discovery of a bright comet by Brooms (*sic*):

1886.	Greenwich M.T.	R. A.	Decl.
	h.	h. m.	° ′
April 30	12 (Midnight)	23 0	N. 21 0

Motion: *slow* towards the *north.*

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 3.

DUN ECHT CIRCULAR, No. 116.

Professor Krueger, of Kiel, communicates the following particulars respecting Brooks' first comet (27th April), computed by Dr. H. Kreutz from observations made on April 29, May 1 and 3:

Elements.

$$T = 1886,\ \text{June}\ 6{\cdot}9585\ \text{Berlin M. T.}$$

$$\left.\begin{array}{rl}\pi-\Omega = & 202^\circ\ \ 55'{\cdot}68\\ \Omega = & 191\ \ \ 48\ {\cdot}58\\ i = & 87\ \ \ 33\ {\cdot}03\end{array}\right\}\text{Mean Equinox } 1886{\cdot}0$$

$$\log q = 9{\cdot}40752$$

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log. distance.	Brightness.
May 5	1	20	56	+57	55·4	0·1196	1·4
9	1	46	37	55	3·9	0·1128	1·7
13	2	9	38	51	43·9	0·1062	2·2
17	2	30	25	47	52·2	0·0996	2·9
21	2	49	32	43	25·9	0·0931	4·1
25	3	7	48	38	17·2	0·0865	6·2
29	3	26	26	32	15·3	0·0794	10·0
June 2	3	47	46	25	9·9	0·0710	16·7
6	4	15	42	+17	16·9	0·0598	23·6

The brightness on April 29 = 1.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 8.

DUN ECHT CIRCULAR, No. 117.

The orbit of Comet B 1886 (Brooks' second comet, announced in *Circular* 115), has been computed by Professor Frisby, and transmitted by code from Boston, U.S. as follows:

Elements.

$$
\begin{array}{rl}
T = & \text{1886, May 4·54 Greenwich M.T.} \\
\pi - \Omega = & \left.\begin{array}{l} 38^\circ \; 45' \\ 287 \;\; 57 \\ 100 \;\; 20 \end{array}\right\} \text{Mean Equinox 1886·0} \\
\log q = & 9·92552
\end{array}
$$

(where the second line of the brace is $\Omega =$ and the third is $i =$)

Ephemeris for Greenwich Midnight.

1886	R. A. h.	m.	s.	Decl. °	′	Brightness.
May 10	23	42	0	N. 43	18	1·02
14	0	6	16	51	32	
18	0	39	8	58	52	
22	1	19	4	64	57	0·72

Comet B was seen at Rousdon Observatory, Devon, by Mr. C. Grover, on May 4 at 15h. G. M. T., in R. A. 23h. 17m., and N. Decl. 30° 40′, very small but very bright, with distinct fan-shaped tail about 45′ long.

Comet A was observed by Colonel G. L. Tupman at Harrow:

1886	G. M. T. h.	m.	s.	R. A. h.	m.	s.	Decl. °	′	″
May 6	10	28	27	1	27	22·1	N. 57	16	29

About 8 mag. round? 2′ diameter, with central condensation, but no nucleus.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 10.

DUN ECHT CIRCULAR, No. 118.

A "Science Observer" telegram from Boston, U.S., gives a further computation of the path of Comet A, based on observations on April 29, May 2 and 5, by Mr. H. V. Egbert.

Elements.

T = 1886, June 7·79 Greenwich M. T.
$\pi - ☊$ = 199° 30′ } Mean Equinox 1886·0.
☊ = 193 35 }
i = 87 53 }
$\log q$ = 9·4547

Ephemeris for Greenwich Midnight.

1886	R. A. h.	m.	s.	Decl. °	′	Brightness.
May 11	1	59	24	N. 53	19	1·19
15	2	20	48	49	44	
19	2	40	16	45	42	
23	2	58	16	41	3	4·48

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 11.

DUN ECHT CIRCULAR, No. 119.

Elements and Ephemeris of Comet B, 1886.

The elements have been computed by Dr. H. Oppenheim from observations on May 2 at Kiel, May 4 at Hamburg, and May 6 at Leipsic. The outstanding differences are: $d\lambda = +6''$ and $d\beta = +5''$.

Elements.

T = 1886, May 4·3998 Berlin M. T.
$\pi - ☊$ = 38° 21′ 41″ } Mean Equinox 1886·0
☊ = 287 35 45 }
i = 100 2 43 }
$\log q$ = 9·92520

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log. Δ	log. r	Brightness.
May 19	0	48	43	N. 60	21·3	0·0192	9·9483	0·8
21	1	11	0	63	19·5			
23	1	36	43	65	52·8	0·0428	9·9608	
25	2	5	53	67	58·9			
27	2	38	8	69	35·8	0·0698	9·9753	0·6
29	3	12	33	70	43·0			
31	3	47	45	71	21·5	0·0983	9·9911	
June 2	4	22	10	71	33·8			
4	4	54	25	N. 71	23·9	0·1270	0·0078	0·4

The eclipse of Jupiter's fourth satellite, as observed at Harvard College on April 30, lasted 30m. 20s. longer than the tabulated time; hence Professor Pickering thinks that there may be another eclipse on May 17.

The approximate time of the middle of the phenomenon will be 11h. 26m. G. M. T., but its duration and the degree of obscuration are of course very uncertain. See also a letter on this subject by Dr. E. J. Spitta, in the *Observatory* for April, 1886.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 15.

DUN ECHT CIRCULAR, No. 120.

Elements and Ephemeris of Comet A, 1886.

The elements are derived from observations at Rome on April 29, Kiel on May 4, and at Leipsic on May 9.

Elements.

$$\begin{aligned} T &= 1886,\ \text{June } 7{\cdot}7066 \text{ Berlin M. T.} \\ \pi - \Omega &= 199^\circ\ 59'\ 35'' \\ \Omega &= 193\ \ 23\ \ 46 \\ i &= 87\ \ 50\ \ 54 \\ \log q &= 9{\cdot}44812 \end{aligned} \quad \left.\begin{matrix} \\ \\ \\ \end{matrix}\right\} \text{Mean Equinox } 1886{\cdot}0$$

Ephemeris for Berlin Midnight.

1886.		R. A.		Decl.		log. Δ	log. r	Brightness.
	h.	m.	s.	°	′			
May 25	3	6	25	N. 38	43·3	0·0933	9·6907	5·6
26	3	10	51	37	22·0			
27	3	15	18	35	57·5			
28	3	19	48	34	29·8			
29	3	24	23	32	58·7	0·0869	9·6045	8·5
30	3	29	5	31	24·1			
31	3	33	57	29	45·9			
June 1	3	39	1	28	4·1			
2	3	44	19	26	18·8	0·0792	9·5138	·4
3	3	49	56	24	30·3			
4	3	55	55	22	39·2			
5	4	2	18	20	46·1			
6	4	9	8	18	52·0	0·0691	9·4523	18·6
7	4	16	26	16	58·3			
8	4	24	12	15	6·3			
9	4	32	24	13	17·1			
10	4	41	0	11	31·8	0·0571	9·4695	18·2
14	5	18	10	N. 5	19·2	0·0481	9·5490	13·1

The brightness at time of discovery is taken as unity.

Dr. H. OPPENHEIM.

Berlin, 1886, May 13.

Mr. G. Harrison of Huddersfield saw that Jupiter's fourth satellite was eclipsed on May 17th, about 11h. 15m. G. M. T., but owing to clouds neither disappearance nor reappearance could be observed.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 25.

DUN ECHT CIRCULAR, No. 121.

Professor Krueger sends the following code telegram: "Brightish unhardness (*unharness*) Brooks undressing May empropriate (*impropriate*) exceptiona (*exceptional*) blanket second zero consummate."

On making the alterations shown in italics the message reads:—

A brightish comet was discovered by Brooks on December (*sic*) 22; its place on May 23, at 15h. G. M. T. is, R. A. 11h. 55m. 12s. and 8° 55′ north declination.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, May 26.

THE PLANETS FOR JUNE.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	3 53 31	N.19 30	5″·4	23 10·1
	9th	5 5 15	N.23 31	5″·0	23 50·2
	17th	6 12 19	N.25 6	5″·2	0 29·6
	25th	7 23 20	N.24 1½	5″·4	1 9·0
Venus ...	1st	1 49 5	S. 8 50½	17″·4	21 6·0
	9th	2 23 19	N.11 47½	16″·3	21 8·7
	17th	2 58 40	N.14 34½	15″·5	21 12·5
	25th	3 35 17	N.17 4½	14″·6	21 17·6
Mars ...	1st	11 10 51	N. 6 23	9″·6	6 30·2
	9th	11 22 46	N. 4 54	9″·1	6 10·7
	17th	11 35 44	N. 3 18½	8″·7	5 52·1
	25th	11 49 37	N. 1 37	8″·3	5 34·5
Jupiter ...	1st	11 48 13	N. 2 48	36″·1	7 7·5
	9th	11 49 21	N. 2 38½	35″·3	6 37·2
	17th	11 51 7	N. 2 25	34″·5	6 7·5
	25th	11 53 31	N. 2 7½	33″·7	5 38·4
Uranus ...	2nd	12 14 25	S. 0 46	3″·8	7 29·7
	18th	12 14 23	S. 0 46½	3″·8	6 26·7

Mercury rises about half an hour before the sun at the beginning of the month, the interval decreasing. After the 11th he sets after the sun, the interval increasing to an hour and a quarter.

Venus rises an hour and a half before the sun at the beginning of the month, the interval increasing.

Mars sets about an hour after midnight on the 1st, and afterwards earlier each night.

Jupiter sets an hour and a half after midnight at the beginning of the month, the interval decreasing.

No. 20 of the *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*—a work commenced in 1878—contains an elaborate paper by Messrs. G. Müller and P. Kempf, on the "Determination of the wavelengths of 300 lines in the Solar Spectrum." A notice of the Observatory at Potsdam appeared in the *Astronomical Register*, XX., 208.

ASTRONOMICAL OCCURRENCES FOR JUNE, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Antares —
Tues	1	1	Conjunction of Moon and Mercury 4° 3′ N. Sidereal Time at Mean Noon 4h. 39m. 34·35s.	1st Oc. D.	11 36	11 40·9
Wed	2	1 55	● New Moon Sun's Meridian Passage 2m. 17·57s. before Mean Noon	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	8 56 10 10 11 12 12 25	11 37·0
Thur	3	22	Conjunction of Moon and Saturn 4° 1′ N.	1st Ec. R. 2nd Tr. I.	9 31 27 11 51	11 33·1
Fri	4			3rd Ec. R.	8 32 33	11 29·2
Sat	5			2nd Ec. R.	12 13 26	11 25·2
Sun	6					11 21·3
Mon	7					11 17·4
Tues	8	19 26 21	☽ Moon's First Quarter Conjunction of Moon and Mars 0° 6′ N.			11 13·4
Wed	9	12 5 10	Occultation of B.A.C. 4043 (6½) Conjunction of Moon and Jupiter 0° 1′ N.	1st Tr. I. 1st Sh. I.	10 49 12 5	11 9·5
Thur	10	13 0 16	Near approach of 38 Virginis (6) Mars at quadrature with the Sun	1st Ec. R.	11 26 12	11 5·6
Fri	11	14	Superior conjunction of Mercury and the Sun	1st Sh. E. 3rd Ec. D. 3rd Ec. R.	8 49 9 54 53 12 30 32	11 1·6
Sat	12		Saturn's Ring: Major axis=37″·54 Minor axis=16″·26	2nd Oc. D.	9 32	10 57·7
Sun	13					10 53·8
Mon	14		Sidereal Time at Mean Noon 5h. 30m. 49·59s.	2nd Sh. I.	9 2	10 49·8
Tues	15		Illuminated portion of disc of Venus=0·698 Illuminated portion of disc of Mars=0·885			10 45·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Antares —
Wed	16	1 38	○ Full Moon Sun's Meridian Passage 0m. 22·98s. after Mean Noon			10 41·9
Thur	17	14 59 16 7 22	Occultation of B.A.C. 6536 (6½) Reappearance of ditto Jupiter at quadrature with the Sun	1st Oc. D.	9 51	10 38·0
Fri	18			1st Tr. E. 1st Sh. E.	9 29 10 44	10 34·1
Sat	19	10 18	Occultation reappearance of B.A.C. 7145 (6½)	4th Oc. D. 2nd Oc. D.	10 10 12 8	10 30·2
Sun	20	12 36 13 55 13	Occultation of B.A.C. 7487 (6½) Reappearance of ditto Conjunction of Saturn and Mercury 2° 16′ N.			10 26·2
Mon	21			2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	8 51 9 6 11 37	10 22·3
Tues	22					10 18·4
Wed	23	12 43 13 47	Occultation of 24 Piscium (6½) Reappearance of ditto			10 14·4
Thur	24	4 35 20	☾ Moon's Last Quarter Uranus at quadrature with the Sun	1st Oc. D.	11 47	10 10·5
Fri	25			1st Sh. I.	10 24	10 6·6
Sat	26	19	Conjunction of Neptune and Venus 0° 28′ S.	1st Ec. R.	9 44 33	10 2·6
Sun	27	22	Conjunction of Jupiter and Mars 0° 59′ S.			9 58·7
Mon	28	12	Conjunction of Moon and Venus 2° 57′ N.	1st Tr. I. 2nd Sh. I.	8 54 11 27	9 54·8
Tues	29			3rd Sh. E.	10 27	9 50·8
Wed	30			2nd Ec. R.	9 20 27	9 46·9
JULY Thur	1	10 6 14	● New Moon Conjunction of Moon and Saturn 3° 51′ N.			9 42·9

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
June 1	−87	33 E.	June 11	−29	46 M.	June 21	+28	1 E.
2	+80	13 M.	12	41	59	22	15	46
3	68	0	13	54	13	23	3	35
4	55	47	14	66	26	24	− 8	39
5	43	33	15	78	39	25	20	53
6	31	21	16	+89	7 E.	26	33	6
7	19	7	17	76	54	27	45	20
8	6	5	18	64	40	28	57	33
9	− 5	19	19	52	28	29	69	45
10	17	33	20	40	15	30	81	59

Moon nearest the Earth, June 5, 11h.
,, farthest from ,, ,, 21, 5h.

Books received :—Rapport Annuel sur l'Etat de l'Observatoire de Paris pour l'Année 1885. Par Mons. le Contre—Amiral Mouchez. Paris : Gauthier-Villars. 1886.—Publicationen des Astrophysikalischen Observatoriums zu Potsdam. No. 20. Potsdam, 1886.—A Treatise on Nautical Astronomy. By John Merrifield, LL.D. Messrs. Sampson Low & Co., 188, Fleet Street, London, 1886.—Atmospheric Refraction. By Edward C. Pickering. Cambridge (U.S.) John Wilson & Son. 1886.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The pages of the *Astronomical Register* are open to all suitable communications Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 283. JULY. 1886.

THE VISITATION OF THE ROYAL OBSERVATORY.

The annual Visitation took place on Saturday, June 5, when the Astronomer-Royal presented his report to the official visitors. The work of the Observatory, which was established—as our readers will remember—solely for the purpose of correcting the calculations of the moon's place, has, within the last few years, been very much extended; in fact, the mass of observations seems to have outgrown the time at the disposal of the computers. Owing to the photographic reductions having interfered with spectroscopic work, observations of the chromosphere and solar prominences have been made on only ten days. The Astronomer-Royal has succeeded in his endeavours to induce "My Lords" to grant the necessary funds for the construction of a larger telescope: it remains to be seen whether the present staff will be sufficient to cope with the extra amount of work that will be thus caused. Our national Observatory, which at one time held an honourable position among well-equipped observatories, has long fallen into the rear; but now it is to be hoped that Greenwich will successfully vie with, if not surpass, the noble observatories of continental Europe and America. The new telescope is to have an aperture of 28 inches, and the grinding of the lenses has been entrusted to Mr. Howard Grubb. The instrument will be mounted on the present south-east equatorial, designed by Sir George Airy.

From the report itself we glean the following:—From observations made in 1885, the value for the co-latitude is 38° 31′ 22″·04, differing by 0″·14 from its assumed value. The correction to the tabular obliquity of the ecliptic is + 0″·81, The mean error of the moon's tabular place (computed from Hansen's Lunar Tables, with Prof. Newcomb's corrections) is +0s.·02 in R.A., and —0·″58 in N.P.D. It appears that the defect in the definition of the Lassell reflector, which was at first supposed to be caused by the tilt of the large mirror, has been traced to its cause—convection currents at the eye-end. With a view to prevent these, a wooden screen has been fixed in such a manner as to protect the open end of the tube from the heat of the observer's body. Good work may now be expected from this celebrated instrument. From measures made of the displacement of the F line, it is concluded that Sirius is still approaching the solar system. A considerable number of similar observations have been made in the case of fifty other stars, with a view to the determination of their motion in the line of sight. With respect to the sun, there were only twelve days during the year ending on May 20 on which its disc was free from spots. The areas of both spots and faculæ have shown a marked diminution in 1885 as compared with 1884, the mean daily area for 1885 being in each case only three-fourths of that for the previous year. The diminution in the spotted area has taken place principally in the northern hemisphere. As regards magnetic observations, the approximate mean declination for 1885 is 18° 2′ west; the mean horizontal force, 3·9376 in British units, corresponding to 1·8156 in metric units, and the mean dip (taking the mean of the mean of the three sized needles employed in the observations) 67° 27′ 49″. The Astronomer-Royal gives some figures respecting the long-continuing cold weather of the spring of the present year. From Jan. 5 to March 18 the mean temperature was 5°·9 below the average. The lowest temperature recorded during the whole of the spring was 16°·5 on January 7. The mean proportion of sunshine for the year was 0·283, constant sunshine being represented by unity. These observations were made with Campbell's sunshine instrument. Jordan's recorder has also been used for a part of the time. A comparison of the results with the two instruments for the month of June, 1885, shows 222 hours of sunshine registered by Jordan's recorder, and 212 hours by Campbell's.

The rainfall in 1885 was 24·0 inches, being 0·8 inch below the average of the preceding 44 years.

The report thus concludes:—"In view of the recent development of astronomical photography, I propose to have constructed, for use with the present 12¾-inch refractor of the south-east equatorial, a combination of a convex flint and concave crown lens, which, when placed about two feet within the focus, would correct the chromatic aberration of the object-glass for the photographic rays without alteration of the focal length. If this plan succeeds, the instrument would then be well adapted for photography, thanks to the firmness of its mounting and the excellence of its driving-clock."

ROYAL ASTRONOMICAL SOCIETY.

Session 1886—87.

The last meeting before the summer vacation was held on Friday, June 11th, 1886.

J. W. L. Glaisher, Esq., *President*, in the Chair.

Commander Jas. Llewellyn Heane, F.R.G.S., Alte Burg,
bei Gross Berkel, Prov. Hanover, Germany,
was balloted for, and duly elected a Fellow of the Society.

Col. Tupman mentioned that 63 presents had been received by the Society since their last meeting.

A vote of thanks was passed to the donors.

Mr. Knobel: Before the papers are read, I should like to call the attention of the Society to the very pleasing fact, that the catalogue of our library, which has been for the last two years in preparation, has been completed, and will shortly be in the hands of the Fellows. (Applause.) The labour has not been a small one; and the catalogue will, I believe, be found to be excellent. Whatever merit it possesses is due entirely to the great energy and patient care which our assistant Secretary, Mr. Wesley, has brought to bear upon it. (Cheers.) It is, I think, a matter of congratulation to the Society, that it has an assistant Secretary who is capable of preparing such a catalogue of our library. (Applause.) The catalogue is arranged in alphabetical order, according to the authors' names, but in certain cases it departs from that plan in giving appendices containing lists of papers published by observers at Greenwich, Harvard College Observatory, the United States Observatory, and others; and there are also lists of bibliographies, ephemerides, and star charts in the library. These have been given as class catalogues, because it was thought they might be useful to Fellows of the Society consulting the library.

Mr. Beck: Is the catalogue to be presented, or is it to be purchased by the Fellows of the Society? It seems to me it will be a very valuable work, and that every Fellow should have a copy of it, so that he may know what he will find in the library.

Mr. Knobel: That matter has been considered by the council, but the expense of producing this volume has been so great, that it was found impossible to present it gratuitously to the Fellows.

The council have however resolved that the cost shall be the nominal one of 2s. 6d. to the Fellows. (Applause.)

Mr. Beck: I think it would not be too much to present those old members who have been regular in their subscriptions with copies.

The President: The council thought that by fixing a nominal sum like 2s. 6d., we should insure that it would come into the hands of everyone who cared about it. If you give away a book like this, some people might take it on the principle that they might as well have what they could get for nothing; but at 2s. 6d. any Fellow who values it will be able to have it.

Captain Noble: Perhaps it will satisfy Mr. Beck to know that the charge to the general public is to be 10s.

Mr. Plummer read a paper by Prof. Pritchard entitled *Supplementary Measures of the Magnitudes of a Zone of Stars near the Equator for Reference as Standards of Magnitude in lieu of Polaris.* He said the measurement of the 50 stars selected by Prof. Pritchard has been made with a different view to that of the American astronomers when they selected their zones. In that case' they merely wished to identify, rapidly and surely, certain small sections of the sky. The object of Prof. Pritchard was first to show that a survey of the photometry of the northern and southern heavens might be carried out uniformly, and, further, the altitude of Polaris is such in these latitudes, that it is not always convenient to compare other stars with it; and so, to avoid any correction for the atmospheric absorption, he selected stars all round the Equator, so that they would have all possible altitudes, and thus stars would be compared without that correction. The observer will be able to select a comparison star of nearly equal altitude with the star observed, and will avoid to a great extent the difficulties arising from differences of altitude between the comparison star and the star observed. It is a satisfaction to know that the great mass of the stars which have now been observed 60 or 70 times differ very little from the magnitudes given in the *Oxford Uranometria.* In the concluding paragraph of the paper, Prof. Pritchard stated that he had been making experiments at Oxford University Observatory as to the application of photography to stellar photometry, and the result of his researches had been communicated to the Royal Society. He believed that it would be found that the photographic method, properly applied, was pre-eminently adapted to photometry for the fainter stars, including all below the 7th magnitude. In making these researches, he had established a very simple law connecting the diameter of the spurious photographic disc with the magnitude of the star. Of course, such a law only held good for comparisons on the same photographic plate. The law might be stated thus: The difference of the diameter of the photographic disc varies as the logarithm of the magnitude of the star—in other words, the diameter of the discs will vary in arithmetical progression when the magnitudes are taken in geometrical progression. His idea was that too large a power should not be used, as it might lead to very indifferent results.

A second paper of Prof. Pritchard, entitled *On a remarkable instance of the detection of Distortion in a Photographic Film, measured for the purpose of Stellar Parallax,* was also read by Mr. Plummer. He said that in view of the important relation recently established between photographic astronomy and observational astronomy any communication, however trifling, relating thereto, so long as it was accurate and sincere, would no doubt be acceptable to the Society. In this view he sent an account of a somewhat remarkable condition in which he had twice found the photographic film, which at once conveyed a warning and encouragement. The case in question was detected in photographs taken for the purpose of determining the parallax of 61 Cygni. The main thing in selecting stars for reference was, they should be stars not too faint, and they should be such as could be photographed with a comparatively short exposure, so that the disc would not be too large. The plates were usually exposed for not more than five minutes, the object being to limit the diameter of the stellar discs to the smallest dimensions. With a much longer exposure the 2 discs of 61 Cygni might probably coalesce. On each of the eight plates, taken on four nights, the distance from the comparison stars had been measured. Those measures, as regards seven of the plates, exhibited very satisfactory results, and presented a probable error not exceeding one-tenth of a second. The fact of the disturbance of the film was not only confirmed but was actually localized by the measurement of stars. The film had been distorted in the neighbourhood of three of the stars, and unimpaired in other parts. From these results he thought they might fairly deduce the reliability of the process, and the general fixity of the photographic film.

Capt. Noble : I should like to know whether we are to understand from the paper that a red and blue star which disappear at the same instant by the method of extinction, show discs of the same diameter on the photographic plate.

Mr. Plummer : The red stars, so far as the observations have gone, have given somewhat smaller discs than the blue ones. Hitherto the measurements have been principally confined to the Pleiades, because it is there that we have the largest number of stars of which the magnitude is accurately known and can be compared.

Mr. Ranyard: I should have thought that this law, which is a very important one if true, can be only approximately so for a certain range of magnitudes. If the law connecting the brightness and the diameter of the irradiation disc were absolutely true, one would expect to find the irradiation fringe around the image of the sun much broader than it actually is. The light of the sun must differ from the light of any star by thousands of magnitudes. This may possibly be an approximate result which holds good for small illuminations.

Mr. Plummer: We have only measured down to the 9th and 10th magnitudes, and our instruments are unfortunately not large enough to enable us to photograph beyond the 13th magnitude. There are, however, photographs by the Bros. Henry, and we hope to be able to measure the discs on those, with a view of seeing how far the formula can be trusted. I do not think I followed Mr. Ranyard in his remarks about the photographic radiation fringe. That is a quantity which we neglected to measure. Our measurements are confined to what I may call the nucleus of the star, in the double image micrometer which I prefer to use for the measurement of the disc, and the fringe is actually lost.

Mr. Common: In taking photographs for the purpose of measurement, I should like to suggest that the difficulty arising from the diameter of the large discs due to the light of bright stars might be got rid of by introducing a diaphragm or bar during the exposure, which would cover the place of the brighter stars, and they might then only be exposed for a second or some short period, which would allow of their being represented on the plate as minute points.

The President: This discussion might, I think, be postponed for a few minutes, because we have the pleasure of a visit to-night from M. Cornu. He can unfortunately only stay a few minutes, and I should like to ask him to give an account of his researches bearing on astronomy; and perhaps Mr. Stone and Mr. Common would not mind postponing their remarks in order to hear him. (Hear, hear.)

M. Cornu said: Will you allow me to offer to the Society two papers of mine?—one concerning the distinction between the lines in the solar spectrum, produced by the absorption of our terrestrial atmosphere, and the lines caused by the absorption of the solar atmosphere. In this memoir, I point out the way in which it is possible on inspection to distinguish immediately if any line is atmospheric, or telluric, or if it is solar. It is necessary to possess a spectroscope of great dispersive power, and with that and other conditions it is very easy to distinguish immediately between the two classes of lines. Previously it was necessary to wait till the sun was high above the horizon, and after that to examine the spectrum again when it was very low; and it was assumed that the telluric lines were those which showed a considerable amount of difference of density or darkness in the two positions of the sun. It was very tiring work to await these conditions, and keep in mind the relative darkness; and if the weather was not very clear, it was necessary to repeat the observation very often. I have employed the method I propose instead of that observation, I project with a lens the image of the disc of the sun on the slit of the spectroscope, so that the eastern equatorial limb of the sun is first thrown upon the slit; this portion of the sun is approaching, and consequently all the solar absorption lines are shifted a little upwards in the spectrum, that is, towards the blue end. Then, by a small swinging motion of the lens, the western equatorial limb is rapidly brought upon the slit, and the solar absorption lines are slightly depressed in the spectrum; the wave lengths in this case being made longer by reason of the motion away from us of the sun's limb, while the absorption lines due to the earth's atmosphere remain identically in the same place. As the eastern and western limbs of the sun are rapidly brought upon the slit by the oscillation of the reflector, the solar lines appear to tremble in the spectrum as compared with the terrestrial absorption lines, which remain stationary. (Applause.) As to the other paper which I have the honour to lay before the Society, it concerns the lines in the spectra of a good number of metals. There is exactly the same distribution of dark lines which Dr. Huggins has discovered in the spectrum of the white stars. These dark lines belong to the hydrogen spectrum, and the same law of distribution is visible in a great number of spectra of metals, for instance, in the spectrum of aluminium, zinc, manganese, and so on. The lines are arranged in the same disposition, not only in distance, but also in density, which is always diminishing in inverse ratio to the wave length. Those lines are characterized by a singular property: they are self-reverting in the spectrum, that is to say, if you put a bit of metal upon a carbon, you get a great quantity of vapour; if there is only a little quantity of metal you see them as bright lines; but if the quantity of metallic vapour becomes greater, you see a continuous spectrum, on which the lines are reversed, and have exactly the appearance of the black lines in the spectra of white stars, as discovered by Dr. Huggins.

The thanks of the meeting were returned to M. Cornu for his paper.

Resuming the discussion on Prof. Pritchard's papers, read by Mr. Plummer,

Mr. Stone said: I only wish to know how these magnitudes have been obtained. Have they been obtained by the wedge photometer, or are they eye-estimations? It appears to me that there is an essential difference between the photometric magnitudes obtained by an absorption process and those which are direct eye estimations. I suppose that in the case of limited apertures, we judge the brightness of the star from the effect upon the eye of the total amount of light condensed by the object glass, but certainly that is not the case in absorption photometry, which leaves out of account a considerable portion of the light which is condensed by the object glass and forms the disc. By the method of extinction light is lost before the disc is cut down to its vanishing state. Some years ago, I computed the intensity of the illumination at different parts of a star disc, by dividing it into 20 equal angular parts, and found the illumination of the central portion of the disc is only about $\frac{1}{84}$th or $\frac{1}{85}$th part of the total amount of light in the disc. Therefore, when we talk about judging of the relative brightness of stars by a method of this kind, it seems to me it becomes a matter of some importance to consider what is the effective portion of the light condensed by the object glass, which is measured by means of the photometer.

Mr. Common: What photometer?

Mr. Stone: By any method of extinction. The point I am speaking of has nothing to do with Prof. Pritchard's method particularly. It refers to all extinction methods. When you are using the extinction method, you lose all the light which falls outside the central portion, and ultimately you lose the form of impression seen by the eye in a vanishing point. I cannot say exactly how the two phenomena are connected, but it is not the total amount of light which forms the disc, and certainly not the whole of the light which is effective upon the photographic plate, which is effective upon the eye through the absorbing medium. Therefore, I think a formula of that kind can only be approximative, and I think there would be a difference between the magnitudes estimated by the absorbing apparatus and by the eye itself directly.

Mr. Ranyard: I should like to ask how Mr. Stone calculated the intensity of the light of the various parts of the disc and rings: was it by the ordinary method of physical optics, and corresponding to what diameter of object glass?

Mr. Stone: Yes; I have calculated it strictly. It was done some years ago when measures of diameters of discs were brought forward, and there was an idea that the diameter of the rings did not vary inversely as the diameter of the object glass.

Mr. Ranyard: Do you make out that the total brightness of the first ring is greater than the total brightness of the disc?

Mr. Stone: No; what I say is if I divide the disc into 20 parts, the illumination of the $\frac{1}{20}$th part at the centre is only $\frac{1}{85}$th part of the total illumination of the disc.

Mr. Plummer: The distances Mr. Stone has been speaking of are very small. I have calculated and measured them, and I believe the large area of the photographic image might be accounted for by diffraction, but it is no such thing. The brilliancy of the third ring is only about $\frac{1}{1000}$th of the light at the centre, and that in the 13-inch equatorial is reduced to a radius of about one second and $\frac{2}{10}$ths. Does Mr. Stone say that the photographic star discs have anything to do with the light of the diffraction rings?

Mr. Stone: No. They have nothing to do with the photographic image. It is the disc as seen by the telescope I am speaking of. The disc I have taken is simply a disc defined from the centre to the first dark ring. That I have divided into twenty equal angular parts, and I have computed the illumination of each of those independently, and the total illumination which falls upon that disc at the first dark ring is $\frac{1}{85}$th of the brightness at the centre.

Mr. Plummer: The extinction method is purely differential.

The Astronomer Royal: I wish to call attention to one aspect of this question. As it seems to me, the eye does not perceive geometrical points, but the eye perceives surfaces, and we have to deal with the diffraction discs in the eye as well as in the telescope. Although I agree that there are these gradations of light, still, if we are not using a very high magnifying power, the eye will see the whole light spread nearly evenly over the disc. The eye is, in fact, a telescope of small aperture, without any magnifying power, while we use in astronomy a telescope with a large aperture with certain magnifying power, and the question is, What magnifying power is being used? We are dealing with the eye as an optical instrument; therefore these calculations of the intensity of the light within these very small areas of the refraction disc of the telescope have no force. Every point of the disc formed by the telescope is spread out by the eye into another disc, and it may bear a very large proportion to the diameter of the disc in the telescope. I have not considered the question in this particular light, but it seems to me that it depends very much upon whether we are using a magnifying power of 1000 or 100, whether the degradation of light from the centre to the

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edge of the disc will have any practical effect in observations, or whether it will not.

Mr. Stone: How about clouds? Anyone who has observed a large star through a telescope and seen a cloud come on, has seen the star reduced in diameter to the most minute point.

The Astronomer Royal: That may be because a high power is used. I am simply saying it all depends upon the size of the disc. If we were using a power of 60 on a 12-inch telescope the two discs formed by the telescope and by the eye respectively would be about the same size. I think it was a 12-inch telescope with which these observations were made.

Mr. Ranyard: I should like to point out that the photographic discs are on a very much larger scale.

The Astronomer Royal: Nobody doubts it. I am not discussing that question.

Mr. Ranyard: These discs and rings subtend perhaps thirty seconds. If it is admitted that the photographic discs are much larger, there can be no connection between them and the phenomenon the Astronomer Royal is referring to. I should like to mention that I have tried experiments to see whether it was possible they might be due to light spreading within the thickness of the film, and have shown that they cannot be. I took photographs with platinum foil, which is perfectly opaque, laid over a part of the sensitive surface, and when a brilliant star or bright surface was photographed, the photographic irradiation was sharply cut off by the opaque film. Therefore these discs are due to light falling upon the front surface of the film, and not to light spreading within the thickness of the film, or to any photographic action spreading within the film.

The Astronomer Royal: I think that Captain Abney has explained that photographic discs are formed by absorption in the photographic film of an action spreading in every direction.

Mr. Ranyard: I know Captain Abney's paper, and do not agree with him: irradiation is not due to spreading within the film. I have not seen any experiments printed which contradict mine.

The Astronomer Royal: It spreads in every direction from a bright point.

Mr. Knobel: There is something very anomalous in the methods of star measuring by absorption and by eye estimation. I might call attention to a paper published in the *Monthly Notices*, in which Mr. Gore has determined the magnitude of the new star in Orion from December to March, and he makes a degradation in the light of that star of three magnitudes, using a binocular, and observing with skill and experience as an eye-observer only; but Professor Pritchard with photometric observations with the wedge makes a variation or degradation of only half a magnitude. This seems so anomalous that we have something to account for.

Mr. Stone: As to the size of the disc mentioned by Mr. Christie I think this point is evident to anyone who has observed a large bright star like Sirius during the time a cloud comes on the disc visibly and distinctly getting smaller and smaller. There is no doubt whatever about that.

The Astronomer Royal: If you take a bright star like Sirius, you do not see the true refraction disc; it is mixed with the first, second, and third rings. I do not think you ever see the true refraction disc of Sirius.

Mr. Stone: I have seen the first ring round Sirius perfectly distinctly.

The Astronomer Royal: I do not believe you would see with a clear sky and a telescope with a large aperture, say anything over 6 inches, the true refraction disc of Sirius, but that it would be so mixed up with the first, second, and third rings that the phenomenon described when a cloud passes over would be accounted for by the fact that as the light diminishes, you would see in time the true refraction disc.

Captain Noble: As a matter of personal experience I may mention that on Tuesday evening I was observing η Ursæ Majoris with the Matthew Transit, and there was a kind of blue haze came over the sky. I saw that star as clearly as I ever saw anything in my life, cut down to the merest point like the point of a pin.

Mr. Common: Mr. Ranyard has said he covered the plate with a strip of platinum foil, and has found that the spreading action has been stopped, and that the disc we see corresponding to a first magnitude star was due to something that could be cut off by the platinum foil. What I wish to know, is if that is exactly what Mr. Ranyard means when he says that he covered the plate with a piece of platinum foil. Whether there would be photographic action underneath it or not?

Mr. Ranyard: I do mean that the spreading action appears to be entirely stopped by the opaque platinum foil, and that the size of the photographic star disc has nothing whatever to do with the size of the diffraction disc and rings; for the larger the aperture of the telescope, the smaller should be the diameter of the discs and rings; but the photographic disc increases with the aperture—that is to say, with the brightness of the image.

The President: The Society have present this evening a Fellow who used to be familiar among us, but who has not visited us for three years—I mean Mr. Marth, who has some remarks he would like to make.

UofM

Mr. Marth: I have here a couple of diagrams which show the ecliptical intersection of many types of cometary orbits. They are intended for the library, so that those Fellows who take an interest in the matter may examine them and see what a variety of curves are produced. As the intersects in the neighbourhood of the sun appear too crowded, the parts of a number of them are specially represented on a larger scale on this other diagram. You will find there the intersects of the three interesting comets of 1843, 1880, and 1882, and also the intersects of the direction of the comet, which became visible during the total solar eclipse of 1882. At the time the comet must have been somewhere in the direction represented by the red line, and in the ecliptical longitudes marked at its various points; and if the comet should ever return, the intersects of its orbit must show at the crossing point of the red line the corresponding longitude. If you scrutinize this a little you will find that it is possible that the comet may be one moving in an orbit similar to those of the comets of 1880 and 1882, which would explain its not having been seen before or after. I will not say more about it, as I want to-night to refer to the intersects of the comets of the present year, which promises to be a rich one in comets, and especially to the case of Olbers' comet of 1815. The orbit has been very carefully investigated by Ginzel, of Vienna, whose memoir, I regret to find, is not in the library. According to his investigation the most probable time of the comet's return to perihelion will be next December, but with a very wide margin, so that it is quite uncertain at what season of the year the return will take place, and it will depend on that whether the comet can be well observed or not. By laying down the intersect of the orbit from the data in the *Monthly Notices*, and by cutting out a half-circle of radius 1, the rim of which represents the earth's orbit, the relative position of comet, earth, and sun may be easily recognized if along the track of the intersect are marked not only the ecliptical longitudes, but also the intervals of time between the perihelion passage and the times corresponding to the different true anomalies. In the case of Olbers's comet these intervals of time are for 30° of true anomaly, about 30 days; for 60°, 70 days; for 90°, 143 days, as they may be found written on the diagram. If you will construct the intersect and half-circle for yourselves, I venture to say you may derive more instruction from it than from an actual model. This case is merely a special case illustrating the use of these intersects. In giving the data for their construction, of course I aim at something further. The more orbits of planets and comets become known, the more difficult it becomes to keep in mind something like a fair representation of the solar system; but by means of the intersects you will find it quite possible to form fair notions of the peculiarities of the orbits and of their distribution in the solar system. The problem of finding the proximities of the orbits is rendered comparatively easy. So long as it was considered necessary as a preliminary step to compute the mutual nodes and inclinations of the orbits, the solution of the general problem might well be despaired of; for when you come to the consideration of, say, 500 orbits, their combinations amount to more than 100,000. But with the help of the intersects even a single investigator, if he has leisure, will be able, in the course of time, to find out all the proximities. Problems connected with the determination of the proximities will become more and more frequent. Take the case of the disintegration of Biela's comet. Has that been due merely to some internal process, or has the comet perhaps overtaken one of the minor planets? What has become of Dr. Vico's comet of 1844? Will it turn up moving in a greatly changed orbit? At what point has its motion undergone such a great change, and which body has produced it? Questions like these will present themselves more and more, and the answers will be rendered less difficult with the help of the intersect-data. But the subject is a large one, which I will not pursue. I wanted tonight merely to call attention to the special case of Olbers's comet. I may perhaps add that you may find it worth while to refer to Olbers's letters of seventy years ago. He had been at Paris and had had repeated discussions with Laplace about the latter's cosmogenitic theories; and also about the capture theory of comets. Herschel had started the theory, and Laplace had countenanced it; but Olbers made the sensible objection that the case of Halley's comet, the only periodical comet then known, was against it, though, considering the abnormal case of the comet of 1770, he would not deny such a possibility; and the new periodical comet of 1815 confirmed him in his objection. So you see the modern discussion about the capture theory is merely a revival of old discussions.

The President: I am sure everyone will admire Mr. Marth's energy, industry, and perseverance, in calculating all these intersects, and drawing these most beautiful diagrams. I see the importance of all the problems Mr. Marth has been alluding to just now, and I see how curious these intersects are as a representation of his system, but I do not see how they help the solution of these questions, and I do not see very clearly how the proximities are obtained from the intersects, and Mr. Marth in his paper has not stated it. I wish he would state exactly and accurately how the drawing of these intersects does give an explanation of these problems. I do not mean now, but in a paper.

Mr. Marth: You will find the formulæ for determining these proximities in my paper in the *Monthly Notices.*

The President: I have no doubt it is my own fault. I only read one paper; there was doubtless another.

Mr. Neison: Mr. President and Gentlemen, I need not tell you with what pleasure I find myself once more in an old familiar scene, surrounded by old familiar faces. Though I have prepared a very long paper, I do not propose to take up the time of the Society by reading it, but will give a short account. Since I have been in Natal astronomers have been freely criticising various papers of mine on the lunar theory. Unfortunately in Natal I had very few of my papers, and none of them relating to these particular subjects, so that I have been unable to refer to my own papers, or to most of those published by other astronomers. Of late, however, I have obtained copies of these publications, and it seems to me the time has now come, to reply to some of the objections which have been raised to my own researches. Now, we will put the point very simply in this way. If you adopt the old direct system of calculating inequalities in the motion of the moon, you arrive at one result. That is the result which was originally arrived at by Hansen, and which had been subsequently arrived at by myself. But if you abandon this direct method of attacking the problem, and adopt the very elegant method introduced by Delaunay, you obtain entirely different results. Now, I may mention another point on which observations throw a certain amount of light upon these questions of theory. If you adopt the simple direct method of considering these problems, which was introduced by Laplace and has been accepted by other mathematicians, you will arrive at results which will enable you, or give a possibility of enabling you, to reconcile the observed motion of the moon with the theoretical motion, that is the motion according to theory. But if you say: "No, the results which have been obtained by M. Delaunay and others, who have adopted his system, must be regarded as complete, you are brought to a state of affairs in which you find that theory and observation cannot be reconciled to one another. Now of course that does not prove that one method of dealing with the subject is more accurate than another. It is possible there is some perfectly unsuspected cause for the variation in the observed motion of the moon; but if you adopt M. Delaunay's method of calculation you can say for certain it is something utterly distinct from anything of which we have any knowledge. In a paper which was brought before the Society some ten years ago, I gave reasons (which seemed to me to be very satisfactory) for believing that Delaunay's method was not accurate, namely, that it was imperfect as it only dealt with a small portion of the whole subject instead of the whole, and I was under the impression that the paper which I brought before the Society, though only a preliminary paper, was sufficient to establish this with a considerable amount of certainty. But of late it has been brought before me that other astronomers have not arrived at the same conclusion and do not see the matter from the same point of view that I did. I consider it quite sufficient to show, that by adapting the old system of direct calculation, using methods of whose completeness there can be no doubt, you were led to a result far more extended than that which you obtained by employing the method of M. Delaunay, a method as to the completeness of which there might be shown to exist some considerable doubt.

However, a considerable number of astronomers looked on the matter in another light, and tacitly said, "You differ from M. Delaunay; we admire his method. You must prove him wrong as well as show you are right. Take his method, go through all the work he has done, and show us the exact places where he goes wrong." That would mean devoting myself to four or five years' calculation on an imperfect basis, leading to incomplete results. I did not take that view. To me it seemed waste labour; it was for those who used Delaunay's methods to prove its accuracy. Finding no other astronomer would take the matter up, in the present paper I have gone into the matter first of all with a view to showing that if you adopt M. Delaunay's method you arrive at a result identical with that which you would obtain if you adopt the ordinary direct method, and leave out nine-tenths of the more difficult part of the work. If you do that, you get exactly the same results as M. Delaunay does. But why should you leave out nine-tenths of the difficult part? Why should you leave out all the difficult terms, and say, "These terms are very difficult to calculate, therefore we omit them, on the ground that if M. Delaunay's method is complete, they must destroy each other." I show in this paper that it is not so. There is no reason for supposing that they will destroy each other. The whole calculations that have been made by MM. Hansen and Pontecoulant, and myself, show that these terms do not disappear. Hence, as they are not given by M. Delaunay's method, it is this method which must be incomplete. Then I turn to M. Delaunay's method, and I show that his method rests upon the assumption of the disappearance of all the powers and products of the two parts of the disturbing function, so that if by any transformation you make the first power disappear, it is assumed you make all the higher powers and products disappear. By a simple transformation, however, I show that these powers and products do not disappear; but if you take them into consideration, you get exactly the same result as you do if you adopt the ordinary direct method, which constitutes the foundation of my own researches. It follows that if you take M. Delaunay's method you simply arrive at an imperfect result, which corresponds to that obtained in the ordinary direct method, neglecting all the powers and products of the disturbed elements representing the action of the sun on the perturbations which are due to the planet; that, I think, constitutes all the paper. I should be glad to hear what these astronomers who have been using Delaunay's method have to say to their consideration.

The President: I presume it is the pleasure of the meeting to thank Mr. Neison for his most interesting paper.

On the superiority of zinc and steel pendulums.

Mr. Buckney: This paper was written in consequence of a statement made by Dr. Waldo at the last meeting. He was good enough to give us an account of some improvements in astronomical clocks, which had been made in Yale Observatory, chiefly under his direction, and he stated the net result was that clock errors had been reduced about 25 per cent. on the best known existing clocks. Of course that is a very important gain if it can be substantiated. Dr. Waldo gave us no figures whatever in support of his statement, which therefore must go as nothing more than an expression of his individual opinion. I think it would have been better if he had given us some figures by which we could verify the conclusions at which he had arrived, but as he has not done so, we can only estimate the value of the improvements by seeing and discussing what they are. In the first place, he proposes that his clock should have a heavy mercurial pendulum, weighing from 45 to 60lbs. The jars containing the mercury, and the pendulum rods, are of steel. In ordinary cases the jars are of iron, but the change to steel is not material. The suspension of the pendulum was on knife-edges, in lieu of being suspended on a spring. The escapement he used was a modification of one of the forms of Denison's gravity escapement. I do not know whether it was any improvement. I am unable to say, as Dr. Waldo gave us very little information about it; but what he did give evidently showed that the improvement would have very little effect on the time of vibration of the pendulum. He did not alter the action of the escapement on the pendulum, but he made it rather easier for the clock train to lift the pallets. The only advantage of that would be that the act of unlocking which is done by the pendulum, might be rather more easy. That I think is all that can be claimed for that part of the matter. He insisted on a very massive support for the pendulum, and there I think every one will agree with him. The pendulum no doubt ought to have a perfectly rigid suspension; and he suspended the pendulum from an arch standing on two piers, similar to the piers of a transit instrument, and round these piers in some way he got an arrangement of steam pipes, by which he could regulate the temperature. Well, most of these things are not new, and the only thing I wish to deal with at the present moment is the pendulum, because I have found considerable errors arise from using a very large bob with a mercurial pendulum. About 18 years ago the matter came very prominently to my notice. I had to make designs for the clock, which is now the sidereal standard clock of the Royal Observatory. The clock was to be constructed according to a certain specification drawn up by Sir George Airy, and one of the points was that it was to have a mercurial pendulum. Recognising the advantage of giving weight to the pendulum, I gave the clock a pendulum of 2¾ inches internal diameter, the pendulum ordinarily in use having a diameter of two inches. At the time this clock was set going, I had some other clocks which were to be used in the transit of Venus expeditions, and for the sake of portability, they had pendulums made of zinc and steel, constructed in what was at that time the ordinary way, that is, part of the compensation of the pendulum was performed by the expansion of the bob. I found all these clocks were very sluggish in their action. They were put in a room in which we had the means of raising the temperature by means of a gas stove. When the temperature was raised it took certainly two days before the clock settled down to the new temperature. Twenty-four hours was the least; but with the larger mercurial pendulum it took nearly two days. I began to think about that, and it was evident that that was owing to the bob itself taking a longer time to accommodate itself to the increased temperature, and I thought that the best way would be, as far as steel pendulums were concerned, to cut the bob out altogether, which I did by supporting it at the centre of gravity, instead of at one end. By supporting it in that way, the expansion of the bob would have no influence whatever on the compensation of the pendulum. That made a very great improvement indeed. The clocks at once went very much better than the Greenwich clock with the large mercurial cylinder. In the end I laid the matter before Sir George Airy, and got his consent to put a zinc and steel pendulum to the Greenwich clock, which it now has. That was put before it went to Greenwich. I made afterwards some experiments with two mercurial pendulums, and a pendulum of zinc and steel. I took in one instance a two inch mercurial jar, and in the place of the steel rod I mounted a glass tube, and I filled the whole with mercury until it stood some distance up the glass tube. Then I put it into hot water of a temperature of perhaps 150°. The first effect of that was to cause the mercury to sink rapidly in the tube, showing that the jar itself had expanded before the mercury took up any increase of temperature. If that took place in the clock, which it would on any change of temperature, although in a much smaller degree, you would have an error due to the change of temperature owing to the steel rod expanding before the mercury acted. I also made another experiment, which has some bearing on the sluggish action of the pendulum, and shows most conclusively the disadvantage of having a large mercurial jar. I had two jars, one two inches in diameter, and one two and three quarters inches in diameter. It was the identical jar which had been made before for the Greenwich clock. I filled these with mercury to a proper height, and put a thermometer into each of them. The thermometer was plunged down into the centre of the mercury. I took at the same time the outer steel tube, or rather a short length of it, and one of the zinc and steel pendulums, and inside that I put a piece of the zinc tube that was used. I had first corked up the lower end of the steel tube so as to prevent the water in which I proposed to put it getting in. Inside the zinc tube I put a thermometer. I put all these three, the two mercurial jars and the piece of tube, in a vessel; in fact, it was a saucepan I got from the kitchen. I filled it with water and put it on a gas-stove, and kept the water at about 200° for a couple of hours, until it was evident that the high temperature had thoroughly pervaded the inside of the bobs. The thermometers all read the same. I took them out of the water and I noticed the time that each one took to cool down a certain definite number of degrees. I noted the time each one took from 180° to 140°. The zinc and steel pendulum cooled down that amount in four minutes; the 2-inch mercurial jar in 21 minutes; the 2¾-inch mercurial jar in 29½ minutes. The times severally occupied in cooling down the next twenty degrees were 5 minutes, 15 minutes, and 22 minutes. In cooling down from 120° to 100° the time occupied was 8 minutes, 24 minutes, and 38 minutes. From 100° to 80° the time was 5 minutes, 19 minutes, and 29 minutes. From 80° to 70°, below which point the observations were not carried, the time occupied was 26½ minutes, 70 minutes, and 115 minutes. The variations were perfectly constant, and gave a ratio of one, three, and five, showing that a zinc and steel pendulum would accommodate itself to a new temperature in one-third of the time that a 2-inch mercurial jar, and in one-fifth of the time of a 2¾-inch jar; and of course the clock errors might fairly be assumed to be in those proportions; but the zinc and steel pendulum has an advantage that is not shown in this way. In ordinary practice the outer steel tube is cut away very much so as to expose as much of the surface of the zinc tube as possible, and the zinc tube itself is pierced with many holes, so as to allow the temperature to reach the internal rod. That would give the zinc pendulum this advantage, that it would tend to make its action simultaneous, a real advantage which did not enter into my experiments, because I wanted to keep the water out of the inside, and of course I could not put holes in it. Dr. Waldo claims as one of the points of his clock, that he has from 45 lbs. to 60 lbs. of mercury. That would mean that one jar was 4-in. in diameter and one 5-in., and these errors seem to be pretty much according to the size of the jars; and therefore his 5-in. pendulum would take about 20 times as long to accommodate itself to a change of temperature as a zinc and steel pendulum. He claims an advantage on account of the extra weight of the pendulum, but you can give that to a zinc and steel pendulum by increasing the size of the bob, which does not enter at all into the compensation; all the other parts remaining the same, as long as you do not overload the zinc which has to carry the weight. I have given in the paper a record of the performance of one of these clocks, which was intended to be used in the transit of Venus expedition. I take it because it was exposed to very great changes of temperature. It was placed in one of the huts to be used in the observations, and was kept going for a period of over six months in the grounds of the Royal Observatory. The record was carefully taken. It was given to me some nine or ten years ago. I think it was sent to me by the Astronomer Royal, Sir George Airy. The temperature of the hut varied very much as the temperature of the external air. There was very little difference between them; for the protection consisted only of thin boards. And the going of that clock, I am sure, is much better than you could get from any mercurial pendulum, certainly with a jar of large size. Of course, in order that you may get a clock to go properly and to keep anything like a rate, you must have all the different parts which perform the duty of compensation acting simultaneously. In the zinc and steel pendulums, I believe the actions to be very nearly simultaneous, inasmuch as the parts are almost of the same bulk. There is not one thick piece acting in one direction, and a thin piece in another. There is very little difference between them. In the mercurial pendulum you have the expansion of the steel rod, which is very small, and you compensate that by the expansion of the mercury in the bob, which is very large, and the one evidently does not act as quickly as the other. The same thing has been observed in thermometers. The bulbs of the thermometers, I believe, used to be made very much larger than they now are. Of course in thermometers, weight does not enter into the question at all. It does not matter whether a thermometer weighs more or less, but in a clock pendulum, weight is an advantage. But if in order to obtain that weight you get sluggish action, you will in my opinion, commit an error which is very much greater than the one you are trying to obviate.

The Astronomer Royal: I should like to ask one question in reference to this. Mr. Buckney has alluded to the question of weight. The first zinc and steel pendulum that was made was for the Westminster clock, and I think it was on account of the great weight of that pendulum that zinc and steel was adopted.

Mr. Buckney: No; that is not so. Zinc and steel was used a very long time before the Westminster clock was made.

The Astronomer Royal: But it was adopted for the Westminster clock.

Mr. Buckney: It was so for the Westminster clock, undoubtedly.

The Astronomer Royal: What was the weight of that pendulum?

Mr. Buckney: Six cwt.

The Astronomer Royal: One great advantage of the zinc and steel pendulum is that you can use a very heavy pendulum.

Mr. Buckney: But with that pendulum the expansion of the weight itself enters into the compensation. The variation of temperature is comparatively small, and it has certain advantages in that way, but the bob itself enters into the question of compensation. It is not performed entirely by the zinc and steel, or rather it is iron in the case of the Westminster clock.

The President: I am sure the Society will thank Mr. Buckney for his paper.

The Rev. F. Howlett read a paper *On the Asserted Foreshortening of the Inner Side of the Penumbra of Sunspots when near to the Sun's Limb.* He said: At this late hour of the evening I will not read my paper *in extenso.* You are all no doubt aware of the theory which Dr. Wilson, towards the end of the last century, started. According to his theory, when a symmetrical circular spot is very near the limb, you see the nucleus displaced towards the inner side of the spot, the shelving outer side of the penumbra vastly predominating; but when a spot arrives at the western limb you will see the reverse effects obtaining. The umbra is then close, or comparatively close, to the eastern side. Dr. Wilson asserts that he saw spots showing this peculiarity so strongly that the penumbra on the inner side had entirely disappeared. He makes most circumstantial statements with respect to what he says he saw; and I should be very sorry to call in question such deliberate statements on the part of an eminent man; but at the same time I am perfectly certain that those effects sometimes are totally wanting. For example: On the 12th April last a peculiarly symmetrical spot, and suited for this investigation, was visible on the sun. I find that during a quarter of a century of observation of solar spots there are not twenty instances in which the spots have occurred so symmetrical as to be suitable for such an observation; but Dr. Wilson gives no measurements, and Sir William Herschell, who says he has seen a similar foreshortening, gives no measurements. But on the 13th April the spot went off the disc. It was about 45 seconds in length, and beautifully symmetrical. I assert that when it got close to the edge of the disc the whole spot was not even five seconds in diameter of longitude. The umbra was perfectly central. There was about two seconds and a fraction on each side; and an impartial observer agreed with me that there was a slightly perceptible excess on the inner side, instead of the contrary. I wrote to Father Perry, of Stonyhurst, remarking that the spot would be close to the sun's limb on a certain day, and asked him to examine it very carefully to see if he could detect any difference in the relative breadth of the penumbra. He wrote word that he thought there was a slight amount favouring Dr. Wilson's theory, but not in any degree sufficient to uphold it. I could perceive no difference. My method of observing the sun is to project it on a screen, usually with an image 33 inches in diameter; but in a case of this sort I produced a disc of six feet, and there every second of arc would be about one-fifth part of an inch, and I could distinctly measure with this little ivory measure. It is astonishing, the beauty of the definition of the sun projected on the screen. There is very little room for error in the statement which I make that I can really estimate the breadth of the penumbra of the spot within one second, or even less On the 2nd of June I found that another spot very suitable would be passing off the disc and I wrote to Mr. Ranyard to ask him to watch with great care to see if he could perceive anything favouring Dr. Wilson's theory. He wrote me that he could not see any difference in the breadth of the penumbra on either side. There was nothing whatever to support Dr. Wilson's theory. Therefore the conclusion we would come to is not by any means that I would wish to assert that Dr. Wilson was in error, or that he made a rash and careless statement, but that such foreshortening is very rare. If you will do me the honour to read my paper you will see how very careful and deliberate Dr. Wilson is in his statements. We must come to the conclusion, I think—with Father Perry—that there are solar spots of very different descriptions. Spörer calls attention to the fact that spots by no means generally present this foreshortening, though it is commonly supposed that they always do. I may safely assert that it is quite the exception when they do.

Mr. Maunder: Dr. De la Rue in his researches on Solar Physics measured a number of the sun spots on photographs, taken with the Kew photoheliograph, and as far as my recollection serves me, one out of every seven proved an exception to Wilson's rule. Though I cannot give any numerical statement, that would be about my own impression from the scrutiny of the photographs taken at Greenwich. We may take it accordingly that about one-seventh seem to show a distinct contradiction to Wilson's rule. But the majority do show Wilson's phenomenon. I have watched a great number of circular spots in their transit across the disc, and I think a very large proportion of them have shown something very much like what Dr. Wilson describes: and this is further intensified by the fact that circular spots are usually surrounded by bright faculæ, which, when near the limb are seen to be decidedly higher than the general disc. I think there is very little doubt that they are cup-shaped in their exterior portions, if not in the interior.

The President: There are a great number of papers which I am afraid we shall not be able to read to night: by Mr. Green, Mr. Bryant and others. I do not suppose you care about taking them at this late hour.

The following papers were taken as read:—

Prof. A. Hall: *Observations of the Satellites of Saturn, and Mars, and of the companion of Sirius, made at the U.S. Naval Observatory, Washington.*

J. E. Gore: *On the orbit of* ζ *Sagittarii.*

L. A. Eddie: *Observations of Comet d,* 1885, *Fabry, made at Grahamstown, Cape of Good Hope.*

R. Bryant: *On Kepler's problem.*

Royal Observatory, Greenwich: *Observations of Comet a,* 1886, (*Brooks*).

A. Marth: *Ephemerides of the Satellites of Saturn,* 1886-87.

N. E. Green: *The Northern Hemisphere of Mars.*

A. C. Ranyard: *Note with respect to the invention of the Achromatic telescope.*

SELENOGRAPHICAL NOTES, JULY, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Lunar Photography. At a time when the camera is absorbing so much attention, and amateurs are tentatively following in the wake of the Brothers Henry in an endeavour to vie with these proficients in the art of planetary and stellar photography, a word may be said in this connection on behalf of the moon which, in spite of the prevailing hobby, is in danger of being ignored. Though more than a generation has passed away since Draper, Bond, Hartnup, De La Rue, Huggins, Baxendell and others surmounted the graver difficulties of lunar photography, the hopeful anticipations as to its ultimate results and development have not as yet been realized. It was the confident hope of Professor Phillips and other selenographers, 30 years ago, that pictures would some day be produced which would portray the moon's visible surface with the same distinctness and accuracy as it is observed with the eye under the best conditions of definition and magnification, so that in all questions of suspected change or in matters relating to physical structure an appeal might be made to records beyond doubt or cavil. Whether these desirable results will ever be even approximately attained by any photographic method at present known may well be doubted, but it is certain that if real progress is to be made the prevailing custom of including the whole of the moon's visible disc in the photograph must be abandoned, and the picture limited to comparatively small areas or to individual formations and their surroundings. There is a tendency in some quarters to grossly exaggerate the selenographical value of existing lunar photographs by maintaining that they are more accurate, exhaustive, and true to nature, as regards any given lunar region, than any possible representation of the same region due to eye and hand. The veriest tyro, provided with a good telescope, may assess the truth of this contention if he will devote a favourable evening when the moon is a day or so past its first quarter to a critical examination of the best photographs, say those taken at Melbourne or by Rutherfurd, at New York. Let him compare the actual Plato, Archimedes, Eratosthenes, Alpetragius, Pitatus, Pico, or other formation, near the terminator at this stage, with the photographic portraiture of them. He will remark that as regards the general form, character, and position of the larger details of moderate brightness, there is little to complain of, but beyond this the result is a very long way from perfection. Dr. C. A. Young, in an interesting article in a recent number of the *Princeton Review,* entitled, "Lunar problems now under debate," insists that "good photographs are more authoritative than any drawings made by eye and hand," and though he admits "that the photographic plate is hardly so penetrating for minute details as a keen eye, yet it is brutally honest, and has no imagination, or preconceived opinions." This so-called "brutal honesty" may be well illustrated by the best photographic representations such well known objects as Pico, Piton, Aristarchus, or indeed of any brilliant lunar spot, which even in the most perfect photographs appear as white blotches without any very definite shape or lineaments. It would be indeed an indifferent observer and draughtsman who could not make a sketch of the details of these brilliant objects more true to nature than any photographic portrait of them that has hitherto appeared. The ascription of this exalted truthfulness to the record of the Camera, even as regards lunar objects of moderate brilliancy, needs no little qualification, while with respect to those of from 6° to 10° of brightness, it is altogether contrary to the fact. Nor is the blotchiness of the brighter features the sole drawback,—not only the "minute details" referred to by Dr. Young, but prominent ridges, gross inequalities of the surface, and other appearances distinct enough to the eye with a low power, for some reason or other, fail to impress their true character on their plate, while the intensity of faint markings and like delicate objects, is frequently increased to such a degree as to give them an altogether factitious importance. This indictment, true as regards the present condition of lunar photography, it may be confidently hoped will ere long be untenable, if those who have the time and optical means at their disposal can be persuaded to turn their attention to the subject, making special studies of limited areas under various angles of illumination, instead of, as has hitherto been the case, attempting pictures of the entire disc, which can only possibly be fairly satisfactory in places. The Camera will then speedily become a useful ally to Selenography, and absolutely necessary to the completeness of all observations relating to lunar details, though the sanguine views maintained by enthusiasts as to the rôle it is destined to play in the future may never be realized.

Kempston, Beds: June 18th, 1886.

SIDEREAL NOTES FOR JULY, 1886.

By J. E. Gore, F.R.A.S.

The following known variable stars may be well observed during the month of July:—

1. S. Ursæ Majoris. R.A. 12h. 38m. 57s., N. 61° 43′ (1886·0). A variable discovered by Pogson in 1853. At maximum the magnitude varies from 7·2 to 8·2, and at minimum from 10·2 to 12·8. The period is about 225 days. A maximum was observed by Sawyer, September 17, 1884; and by Baxendell, May 5, 1885; and a maximum will be due this year about July 26. It is a very red star, and lies about 1½° south of the 6 mag. star 76 Ursæ Majoris.

2. R. Boötis. R.A. 14h. 32m. 10s., N. 27° 14′ (1886·0). The maximum magnitude varies from 5·9 to 7·8, and the minimum from 11·3 to 12·2. The period is about 223 days. A maximum was observed by Baxendell, May 13, 1885 (7·8 mag.), and a maximum will be due this year about July 27. The star lies about 2° *preceding*, and a little south of ε Boötis.

3. μ Cephei. R.A. 21h. 40m. 2s., N. 58° 15′ (1886·0). The "Garnet Sidus" of Sir W. Herschel, found by him to be variable from 4m. to 5m. with an irregular period. The variability was confirmed by Argelander, but Schönfeld seems to think it questionable. My own observations during the last three years show that the star is certainly variable to the extent of over 1 magnitude, but the variation seems very irregular. From Argelander's observations—1848 to 1864—Schönfeld derives a period of about 432 days, but my observations do not confirm this. On September 4, 1883, the star was little, if anything, brighter than 5 mag.; while on May 11, 1885, it was but little fainter than ζ Cephei, or about 3·6 mag. At present the star seems again fading slowly.

The spectrum of this star is a remarkably perfect one of Secchi's (3rd) type.

The following "suspected" variable stars may be especially recommended to the attention of observers:—

1. δ Ursæ Majoris. R.A. 12h. 9m. 29s., N. 57° 42′ (1880·0). Schmidt's observations show a variation of 4 or 5 steps, with a period of about 318 days. I am not aware that the variability has been confirmed by any other observer.

2. Birmingham 290. Canum Venaticorum. R.A. 12h. 39m. 29s., N. 46° 6′ (1880·0). This star deserves careful observation, as it would seem to be certainly variable. Birmingham's observations, 1872–1876, range from 4·5 to 6·5. Schmidt finds a

period of about 380 days, with a maximum, April 2, 1876, and a minimum on August 15. It is B.A.C. 4287. The star is of a fine orange colour, and was called by Schjellerup "La Superba." The spectrum is of Secchi's fourth type.

3. Struve 1932 Coronæ (1 Bode). R.A. 15h. 13m. 11s., N. 27° 16′. A close double star and suspected binary. Struve suspected the components of variability, and estimated them 5·6, 6·1; Secchi, 6, 6·5; Dembouski, 6·9 and 7·2. Judging from its brightness as a single star, Dembouski's magnitudes are certainly (at least at present) the most correct. My own observations of this star with a binocular, 1884–1886, show a well-marked variation of about half a magnitude. It was at, or near a minimum (about 6·8) on March 21, 1884; and near a maximum (about 6·3) on March 12, 1885, and April 6, 1886.

4. τ^4 (17) Serpentis. R.A. 15h. 30m. 54s., N. 15° 30′ (1880·0). This red star was rated 7·5 mag. by D'Arrest, and 6 mag. by Secchi. Birmingham's observations, 1872–1876, vary from 6—6·5 to 8 mag., and on one occasion (April 7, 1872) he could not see it! It was measured 6·89 with the meridian photometer at Harvard (U.S.A.), and 6·65 with the wedge photometer at Oxford (1884, 4·91). My own observations, 1878–1886, show small fluctuations of brightness, but they are not sufficiently numerous to decide the question of variation. Schjellerup thought it certainly variable, and found a spectrum of Secchi's (3rd) type, "with narrow zones, like α Orionis."

5. α Lyræ (Vega). R.A. 18h. 32m. 53s., N. 38° 40′ (1880·0). There is a suspicion of variability with reference to a distant companion to this bright star. Position about 42°, and distance 139″. In October, 1870, it was about a magnitude fainter than Herschel's well-known companion (10·5 mag.: 154·9: 48″: 1878, Burnham). Admiral Smyth refers to a small star in the *n.f.* quadrant, which may be the one in question. Observers possessing telescopes of over 4-inches aperture would do well to observe this star occasionally, and note the brightness of the companions above referred to.

6. 53 Draconis. R.A. 19h. 9m. 24s., N. 56° 39′ (1880·0). Sir W. Herschel gives the sequence 47—54, 53. I have found 53 sometimes brighter and sometimes fainter than 54 Draconis. The Harvard measures make 53 slightly brighter than 54, while the Oxford measures make 53 slightly the *fainter* of the two.

7. Birmingham 610 Cephei. R.A. 22h. 18m. 37s., N. 55° 21′. Estimated 7·2 mag. by Argelander, and 6·5 by Schjellerup and Birmingham (March 19, 1873). "On January 17, 1868, Secchi found this star very small, and suggested variability. On December 9 he searched twice, and found that the largest star

on the field of view was no more than 8 mag." On February 17, 1884, I estimated the star 7½ mag. with the binocular. It lies about 1½° *s.f.* ε Cephei.

With reference to Birmingham 118, about which Mr. Gemmill inquires in the June number of the *Astronomical Register*, I have the following observations:—March 6, 1885, estimated 6·9 mag.; March 9, 6·8 mag.; November 30, 6·8 mag.; March 6, 1886, 6·8 mag.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.
To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE RED SPOT ON JUPITER.

Sir,—The red spot is still just within the grasp of small instruments. The following estimations of the times at which it was seen to be on the central meridian of Jupiter's disc were made with a 2¾-inch refractor, a power of 102 being usually employed.

			Time.		Feature observed.
			h.	m.	
1886.	April	26	9	54	Following end.
	„	30	12	38	Centre.
	May	3	9	39·5	Preceding end.
	„	3	10	8·5	Centre.
	„	3	10	36·5	Following end.
	„	20	9	7	Centre.
	„	20	9	36	Following end.
	„	29	11	10	Preceding end.
	June	1	9	9	Centre.
	„	1	9	37	Following end.
	„	15	10	8	Preceding end.

These estimations show that the red spot now follows Mr. Marth's assumed zero meridian by a few minutes only, so that the rate of the spot has been maintained pretty regularly lately. They cannot of course claim any great exactness, on account of the small instrumental means employed, and the faintness of the object; but with this telescope the outline of the spot could be distinctly traced under favourable conditions of definition, &c., except on the south, where it was apparently commingled with the dark belt immediately above it, and occasionally even a reddish colour could be made out. Latterly the red spot has appeared more difficult, perhaps from Jupiter's less favourable position.

Yours faithfully,
West Brighton : A. STANLEY WILLIAMS.
June 19th, 1886.

THE PLANETS FOR JULY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	8 8 54	N.21 53½	5″·8	1 30·8
	9th	8 58 27	N.18 3	6″·6	1 48·8
	17th	9 35 25	N.13 51½	7″·6	1 54·2
	25th	9 58 57	N.10 5½	8″·6	1 46·2
Venus ...	1st	4 3 35	N.18 43	14″·1	21 22·2
	9th	4 42 25	N.20 30½	13″·5	21 29·5
	17th	5 22 21	N.21 46	13″·0	21 37·9
	25th	6 3 9	N.22 25	12″·5	21 47·1
Mars ...	1st	12 0 33	N. 0 18	8″·0	5 21·8
	9th	12 15 49	S. 1 31	7″·7	5 5·6
	17th	12 31 47	S. 3 23½	7″·4	4 50·1
	25th	12 48 26	S. 5 18	7″·2	4 35·2
Jupiter ...	1st	11 35 41	N. 1 52	33″·1	5 17·0
	9th	11 59 3	N. 1 28½	32″·5	4 48·9
	18th	12 3 24	N. 0 58	31″·8	4 17·8
Uranus ...	4th	12 15 11	S. 0 52½	3″·8	5 24·6
	16th	12 16 18	S. 1 0½	3″·6	4 38·6

Mercury sets about an hour and a half after the sun, at the beginning of the month, the interval decreasing.

Venus rises about an hour and a half before midnight, at the beginning of the month, the interval slightly increasing.

Mars sets about half an hour before midnight, the interval increasing.

Jupiter sets half an hour before midnight on the 1st, and afterwards earlier each night.

DUN ECHT CIRCULAR, No. 122.

Elements and Ephemeris of Comet *a*, 1886 (Brooks 1).

The elements are based on observations made at Rome on April 29, at Kiel on May 4 and 10, and at Berlin on May 21.

Elements.

T = 1886, June 7·4481 Berlin M. T.
$\pi - ☊$ = 201° 5′ 7·8″ ⎫
☊ = 192 47 46·8 ⎬ Mean Equinox 1886·0
i = 87 45 6·0 ⎭
$\log q$ = 9·43365

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log. Δ	log. r	Brightness.
June 2	3	45	28	N. 25	55·1	0·0762	9·4992	14·5
4	3	57	30	22	10·3	0·0713	9·4597	17·8
6	4	11	19	18	18·8	0·0652	9·4365	20·3
8	4	27	9	14	30·9	0·0595	9·4372	20·9
10	4	44	50	10	57·4	0·0534	9·4614	19·2
12	5	3	49	N. 7	45·0	0·0482	9·5015	
24	7	0	11	S. 4	33·7	0·0617	9·7627	4·6

Dr. H. OPPENHEIM. Berlin, 1886, May 26.

Lord Crawford's Observatory, Dun Echt: 1886, June 4.

DUN ECHT CIRCULAR, No. 123.

Elements and Ephemeris of Comet *c*, 1886 (Brooks 3).

The elements are based on observations made at Albany, Vienna and Lyons, on May 25, 28 and 31 respectively.

Elements.

T = 1886, June 1·7571 Greenwich M. T.

$$\left.\begin{aligned} \pi - \Omega &= 173^\circ\ 8'\ 6'' \\ \Omega &= 46\ \ 55\ \ 52 \\ i &= 16\ \ 21\ \ 56 \end{aligned}\right\} \text{Mean Equinox } 1886{\cdot}0$$

$\log q$ = 0·17274

Ephemeris for Berlin Midnight.

1886.	R. A. h.	m.	s.	Decl. °	′	log. Δ	log. r	Brightness.
June 4	12	10	31	N. 1	30·7	9·9163	0·1729	0·9
5	12	12	21	0	52·8			
6	12	14	13	0	14·9			
7	12	16	6	S. 0	22·9			
8	12	18	1	1	0·7	9·9249	0·1736	0·9
9	12	19	58	1	38·3			
10	12	21	56	2	15·9			
11	12	23	56	2	53·3			
12	12	25	57	3	30·7	9·9344	0·1750	0·8
13	12	28	0	4	7·9			
14	12	30	4	4	44·9			
15	12	32	10	5	21·7			
16	12	34	17	5	58·3	9·9448	0·1769	0·8
17	12	36	26	6	34·7			
18	12	38	36	7	10·9			
19	12	40	48	7	46·9			
20	12	43	1	8	22·7	9·9562	0·1794	0·7
21	12	45	15	8	58·2			
22	12	47	31	9	33·4			
23	12	49	48	10	8·4			
24	12	52	6	10	43·1	9·9684	0·1825	0·7
25	12	54	25	11	17·5			
26	12	56	46	11	51·6			
27	12	59	8	12	25·4			
28	13	1	31	S. 12	58·9	9·9813	0·1861	0·7

The brightness on May 25 has been taken as unity.

Dr. H. OPPENHEIM. Berlin, 1886, June 6.

Lord Crawford's Observatory, Dun Echt: 1886, June 8.

ASTRONOMICAL OCCURRENCES FOR JULY, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Vega. —
Thur	1	10 6 14	● New Moon Conjunction of Moon and Saturn 3° 51′ N. Sidereal Time at Mean Noon 6h. 37m. 51·06s.			11 53·3
Fri	2		Saturn's Ring: Major axis=37″·33 Minor axis=15″·59	1st Tr. I.	11 4	11 49·4
Sat	3	1 14	Conjunction of Moon and Mercury 4° 35′ N. Conjunction of Saturn and the Sun			11 45·4
Sun	4		Sun's Meridian Passage 4m. 6·36s. after Mean Noon	1st Sh. E.	9 2	11 41·5
Mon	5					11 37·6
Tues	6	19	Conjunction of Moon and Jupiter 0° 33′ S.	3rd Tr. E.	9 4 0	11 33·1
Wed	7	1	Conjunction of Moon and Mars 2° 1′ S.			11 29·7
Thur	8	1 18	☽ Moon's First Quarter			11 25·8
Fri	9	2	Conjunction of Uranus and Mars 0° 34′ S.			11 21·8
Sat	10			1st Oc. D.	10 9	11 17·9
Sun	11			1st Sh. I. 1st Tr. E.	8 42 9 47	11 13·9
Mon	12					11 10·0
Tues	13			2nd Tr. I.	10 44	11 6·1
Wed	14			2nd Oc. D.	9 27	11 2·1
Thur	15	15 8	○ Full Moon Illuminated portion of disc of Venus=0·797 Illuminated portion of disc of Mars=0·888			10 58·2
Fri	16	15 44 16 55	Occultation of B.A.C. 7097 (6) Reappearance of ditto	2nd Sh. E.	8 42	10 54·3

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Sat	17		Sidereal Time at Mean Noon 7h. 40m. 55·96s.	3rd Ec. R.	8 22 38	Vega. 10 50·3
Sun	18	15 30 16 1	Near approach of e^2 Aquarii (5½) Near approach of e^1 Aquarii (6)	1st Tr. I.	9 28	10 46·0
Mon	19		Sun's Meridian Passage 6m. 0·74s. after Mean Noon	1st Ec. R.	9 57 48	10 42·5
Tues	20					10 38·5
Wed	21					10 34·6
Thur	22		Saturn's Ring : Major axis=37″·48 Minor axis=15″·63			10 30·7
Fri	23	19 21	☾ Moon's Last Quarter	2nd Sh. I. 2nd Tr. E.	8 34 9 9	10 26·7
Sat	24	12 24	Occultation reappearance of μ Ceti (4)	3rd Oc. R. 3rd Ec. D.	8 13 9 51 59	10 22·8
Sun	25					10 18·9
Mon	26	12 49 15 6 15 43 15 23	Occultation reappearance of 85 Tauri (6) Occultatiou of σ^2 Tauri (5) Reappearance of ditto Near approach of σ^1 Tauri (5½)	1st Oc. D.	8 35	10 14·9
Tues	27			1st Tr. E. 1st Sh. E.	8 13 9 15	10 11·0
Wed	28	10 11	Conjunction of Venus and μ Geminorum 0° 6′ N. Conjunction of Moon and Venus 3° 46′ N.			10 7·1
Thur	29	6	Conjunction of Moon and Saturn 3° 45′ N.			10 3·1
Fri	30	17 25	● New Moon	2nd Tr. I.	9 5	9 59·2
Sat	31			3rd Oc. D.	9 2 6	9 55·3
AUG. *Sun*	1	4	Conjunction of Moon and Mercury 3° 5′ S.	2nd Ec. R.	9 0 5	9 51·3

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
July 1	+85	48M.	July 11	—36	27M.	July 21	+21	21E.
2	73	35	12	48	39	22	9	8
3	61	22	13	60	53	23	— 3	6
4	49	7	14	73	7	24	15	19
5	36	54	15	85	19	25	27	32
6	24	40	16	+82	27 E.	26	39	45
7	12	27	17	70	14	27	51	59
8	0	14	18	58	0	28	64	12
9	—12	0	19	45	48	29	76	25
10	24	13	20	33	34	30	88	38
						31	+79	9M.

Moon nearest to Earth, July 3, 5h.
,, farthest from ,, ,, 18, 19h.
,, nearest to ,, ,, 31, 11h.

Erratum on p. 135 of *Astronomical Register* for June. In the report of Capt. Noble's remarks : " I should like to ask Dr. Bischoff one question. He referred incidentally to some researches of Dr. Fisher," should be, " I should like to ask Dr. Dreyer one question. He referred incidentally to some researches of Dr. Bischoff."

Errata (Vol. XIX.). On p. 234, the fifth line from the bottom, the angle for April 1st should be 23° 44′, *not* 29° 44′. On p. 235, the angle for July 23 should be 323° 51′ *not* 320° 51′. On the same page, the angle for August 27 should be 31° 24′, *not* 24° 1′.

Books received.—Astronomische Nachrichten.—Observations of Variable Stars. By Ed. C. Pickering.—Accurate Mountain Heights. By the same.—New Form of Polarimeter. By the same.—Twentieth Report of the Board of Visitors of the Melbourne Observatory. John Ferres, Melbourne.—Report of the Oxford University Observatory.—Report to the Board of Visitors of the Royal Greenwich Observatory.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1886.

Allen, Rev. E.
Dansken, J.
Pritchard, Rev. Professor.

To Dec., 1886.

Banks, Mrs.
Böe, A. de
Grove, S.
Williams, A. S.

To June, 1887.

Bennett, W.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 284. AUGUST. 1886.

THE PART TAKEN BY LARGE INSTRUMENTS IN ASTRONOMICAL OBSERVATIONS.

A Lecture delivered at the Sorbonne on March 6th.

By M. Wolf, Member of the Institute.

(*Translation.*)

During the past year a very lively debate has been carried on by astronomers in England and America on the subject of the utility of large instruments for the observation of stars. On one side the amateurs claiming that with their small telescopes of 6·3 inches aperture they can see all that the largest telescopes show, and even observe details which escape the possessors of these enormous instruments; on the other, nearly all attached to the American Universities maintaining the superiority of the great telescopes. There was a sufficiently lively exchange of words: for the first, the gigantic telescopes of the American observatories were not much more useful than the immense eye-glasses which opticians exhibit as a sign, and were really only for show; the second ironically answering that if their great telescopes failed to show the marvellous and rapid changes which their opponents had discovered on the discs of the planets, it was because they had no existence. It has seemed to me that at a time when observatories and private individuals are striving who shall possess the biggest telescope—at a time when the generous munificence of M. Bischoffsheim has endowed the Nice Observatory with a telescope of 79 feet focus and 29·5 inches in diameter, it will be interesting to bring the question before you, to present to you, in discussing them, the arguments and facts advanced by the contending parties, and to try to deduce a reasonable conclusion from them. Experience and theory will guide us in seeking for the truth.

We must first consult the facts: What are the large instruments already constructed? What results have they given? and if mistakes and deceptions have occurred in employing them, what have been the causes of them? Is it possible to avoid them? Let us, then, in the outset, with your permission, study the teachings of astronomical history, and following the method at present in vogue, not contenting ourselves with documents entombed in learned and formal collections, consult anecdotic history; the English pleadings have been adorned with numerous piquant and instructive facts; slanderous tongues have not hesitated to utter loudly what their predecessors spoke very softly; probably there is some profit to be derived from their utterances.

Towards the end of the 18th century the aperture of instruments employed always remained very small. If the telescopes were very long they were very small, and during the last years astronomers had even abandoned the object glasses of 6, 7, and 8 inches of Cassini and Hevelius, to limit themselves to telescopes of only an inch or two in aperture. Thus many details which their predecessors had perceived, became invisible to them. Reflectors alone preserved reasonable dimensions. You know how, in 1780, one of the greatest geniuses that have honoured astronomical science, W. Herschel, effected a complete revolution in the art of observation. The glass-makers could only furnish small pieces of glass: he made telescopes with metallic mirrors, and by successive trials, arrived at the construction of one of the most celebrated instruments of this description, of which the mirror was 4·82 feet in diameter* and the tube 41 feet long. One cannot read without profound admiration the account of the ingenious efforts by which Herschel came to construct and set to work so gigantic an instrument with his own hands. The industry of that date could not supply him the raw material of sheet iron, save in very small sizes, and of a few square décimètres in area. The tube was made of sheet-iron, supported by a forged frame-work. It was set up suspended by two cables between a couple of uprights, formed of two immense double ladders, themselves carried by a movable horizontal frame by means of cast iron rollers running on a circular railway laid on cross bars of wood covered with sheet-iron. The mirror itself rested on rails running diametrically, and a windlass actuating the tackle served to incline the tube at the required height, and to set the whole frame-work in motion. The observer, suspended in a movable balcony before the mouth of the telescope, was in constant communication, by a speaking tube, with his assistant placed in one of the small covered buildings below.

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* This is an obvious error. The real diameter of Sir W. Herschel's largest mirror was exactly 48 inches.—TRANSLATOR.

What it is most important for us to know, are the results obtained with this instrument by the most indefatigable of observers: Without going so far as to say, with Baron Zach, that the 40-feet telescope was of no use at all, which had never served to make a single discovery, and which should be regarded simply as an object of curiosity, it must be candidly admitted that this instrument was not one of those which were most useful to Herschel. Arago found nothing to quote, as having been discovered with this great telescope, but the two interior satellites of Saturn. The observations of the satellites of Uranus were made with 20-feet telescopes; and you will not forget that of the six satellites discovered by Sir W. Herschel, two only have any real existence. In the observations of zones for gauging the heavens, and for the discovery of nebulæ, it is the 20-feet telescope which was always used, the diameter of the speculum was 18·5 inches. The observations of stars were made with an instrument of 7 feet focus of which the aperture of the mirror was only 6·2 inches, and which seems to have possessed great optical perfection. I borrow from W. Struve these details of such interest in the present discussion. But it is important to know why the great telescope was not more frequently employed. The first reason was the difficulty of moving the instrument about. Its movement involved the efforts of two men at least; besides an assistant, who took the time, and wrote down the observations. A second, and much more serious reason, was the bad quality of the images: if we may believe Mr. Proctor, it was a matter of public notoriety in England, that the great 4-feet mirror "bunched a star into a cocked hat." This is a point to which we shall return. However, Herschel's son thought it useless to preserve the telescope as an instrument of observation, since he sealed it up in the garden of his residence at Slough, on January 1st, 1840; and in his expedition to the Cape of Good Hope, he always used a 20-feet telescope, differently mounted to the great one.

Nevertheless, large mirrors, by the enormous quantity of light they collect, present such advantages over small ones, giving the hope of such discoveries, that their use has not been abandoned in their native country, England. Lord Rosse, in 1845, after numerous and difficult attempts, constructed a mirror of six feet in diameter and 55 feet focus. The Leviathan of Birr Castle has been at work ever since this date in the hands of celebrated astronomers, and its name will remain attached to the discovery of a very interesting class of nebulæ—the spiral ones. As a light-grasper it has no rival. Is it as perfect in point of shape? They say that a foreign astronomer who had visited the Birr Castle Observatory, and had taken a glance through the big telescope, summed up his opinion in these words: "They showed me something which they told me was Saturn, and I believed them!" I shall not tell you the name of this illustrious astronomer, out of respect for his memory, as well as for that of the celebrated maker of the mirror. I have only recalled this speech to caution you against the hasty judgment that one is too much inclined to form on large instruments upon a cursory examination. Certainly, at the instant when Saturn was unrecognizable, the mirror of the Leviathan was in bad condition, and the foreign astronomer recognized this by certain appearances. But does it follow that this mirror was essentially defective, and that it had always been so? We shall study presently this question of large mirrors, and we shall see that it is the influence of effective conditions on the form of their surface, and how even a small change in temperature may turn a good mirror into a bad one. But this is not all. The mirror, or rather the two mirrors, of Birr Castle are made of speculum-metal; after a year of observation their surface has to be re-polished, a long and difficult operation, which necessarily changes its form. It is performed three miles from the observatory, and it takes 25 to 30 men, attached to a truck, to transport, without shake, from the workshop to the place of observation, this mirror weighing nearly four tons. It is not until after it has been brought back that the mirror can be turned on to the sky, and it is necessary to wait for a fine night to try it. You will understand that, under such circumstances, astronomers may often be tempted to leave even an imperfect mirror in its place; and far from blaming them, far from intending to disparage their large instrument, we can truly only praise and admire the indefatigable perseverance which animates those scientific men who employ so inconvenient an instrument, and have succeeded in obtaining important results with it.

However, as we must judge large telescopes, we will examine what that at Parsonstown has produced. It has revealed the existence of spiral nebulæ, and has contributed with spectral analysis to solve the question of true nebulæ, not composed of stars. It has at the same time furnished important records for the problem of the variability of nebulæ. But is it destined to afford the definite solution of this problem? Here are two drawings of the celebrated nebulæ in Canes Venatici, obtained from this instrument, the one about 1850, the second in 1878. It almost seems as though one has the images of two different objects before one's eyes. And meanwhile, for no astronomer is the question of the variability of this nebula decided. Furthermore, a mirror of 47 inches a 16-inch mirror even suffices to show all the details of this nebula; and more sharply perhaps than the monster one of Birr Castle. We thus enter into the very essence of the question; of what good are these enormous instruments, so costly to construct, so inconvenient to manage, if it be true that a telescope of 16 inches in aperture, and 95 inches long, can supply their place?

Contemporaneously with Lord Rosse in Ireland, Lassell resuscitated these reflectors in England. With a telescope of 2 feet in aperture, and of 20 feet focus, he discovered, in 1847, the satellite of Neptune, in 1848, the eighth and smallest of Saturn's satellites, Hyperion, and in 1851, the two interior satellites of Uranus. He then constructed the telescope of 4 feet in aperture, of 37 feet in focal length, which equatorially mounted was employed at Malta in the discovery and observation of nebulæ, and, an important thing from our point of view, in making a great number of micrometrical measures of Saturn and his satellites, and of those of Uranus, and Neptune. You will remark that Lassell's great discoveries were not made with his most powerful instruments.

To the number of telescopes with metallic mirrors, we must, in fine, add that at Melbourne, constructed by Grubb, and installed in 1870. The mirror is 4 feet in diameter, and 28 feet focus; it weighs 1·56 tons. The telescope is of the Cassegrainian form, which admits of a more convenient form of equatorial mounting than that of Lassell. This telescope after various misfortunes, has given fine photographs of the moon, and has been chiefly employed in the observation and drawing of nebulæ.

While England thus developed the power of reflectors with metallic mirrors, a revolution was produced in the art of the glass-founder, from which arose another in the optician's art. The first was the work of a simple Swiss workman Guinand, born at Brenetz, in 1744. Successively a maker of clock-cases, next founder, then optician, he sought and found the means of obtaining, free from defects, discs for object-glasses larger than those that the English glass-founders could furnish. The success of his work caused him to be invited by Reichenbach, and Utzschneider, to the celebrated optical institution of Benedictbeuer near to Munich, where he worked with the illustrious Fraunhofer. After his death, his son, with Bontemps, at the glass-works at Choisy-le-Roy, succeeded in producing discs 22 inches in diameter. At the dissolution of partnership, Bontemps went to England, and communicated to Chance of Birmingham the process of manufacture. For some years the house of Chance in England; Feil, grandson to Guinand, in Paris; and in Bavaria, Merz, of Munich, furnished all the large glasses for objectives. At present, M. Feil has got the start, and the largest discs of crown and flint required for object-glasses have been cast in Paris. The latest are of nearly 39 inches in diameter.

But to have the mere glass is not all that is needed. The lenses must be worked to such forms that the image which they give shall be faultless. Mathematicians have calculated the curves which four surfaces of an objective must receive, in order that rays of the same mean refrangibility—the yellow for example—shall coincide at the same focus, those passing through the centre of the lens, as well as those traversing its marginal regions. It is this which constitutes aplanatism; and further, that the rays of two different colours, arbitrarily selected, shall be coincident at the same focus; this constitutes achromatism. The optician gives the required curves to the glass which he is working in the best way possible; but formerly, and often even now, he has only one means of judging the result of his work, that of directing the telescope to the heavens when he is finishing the objective. But when it becomes a question of making, as astronomers now require, object-glasses of even 66 feet in focal length, it is necessary for the optician to know in what direction he is working to put his glasses weighing many hundreds of pounds, at the end of a tube 66 feet long, which he can direct at will towards the most favourable part of the sky. Foucault found himself in the presence of such a difficulty when Le Verrier requested him, in 1855, to turn two discs of crown and flint glass, purchased of Chance in Birmingham, into an object-glass of 28·7 inches aperture, and 52·5 feet focus. In 1859, Foucault published, in a Memoir which has now become classic, the whole of the admirable methods by which he succeeded in certainly giving to the surfaces of the glass the curve required by theory: thus constructing in the workshop, mirrors and object-glasses which could be immediately mounted as reflectors or refractors, and giving in the outset perfect images. An immense advance in the old methods, where the successive attempts of the optician sometimes spoilt the mirror, or the objective, when it had nearly attained perfection! More greatly improved still by M. Ad. Martin, the methods of Foucault are so certain, that they have enabled MM. Henry, two already celebrated young astronomers at the Observatory, to construct, with a truly marvellous rapidity, mirrors and object-glasses which defy all foreign competition.

You know, gentlemen, how L. Foucault's first attempts brought him perforce to the construction of parabolic surfaces, and how, in the presence of his first results, which gave him perfect and durable telescopes by covering the glass with a thin film of silver, he forgot object-glasses for the time being. Reflectors of 15·75 inches, 19·69 inches, and 28·5 inches were made; and it was with this last, while it was still in Paris, that the companion to Sirius was seen, for the first time in Europe, by M. Chacornac. This telescope is now at Marseilles, where it is used by M. Stephan with indefatigable zeal in searching for small nebulæ. Two other mirrors, one of 30·5 inches, by MM. Henry, the other of 40 inches, by M. Ad. Martin, have been mounted at Toulouse, and in Paris. Have these great instruments done what was expected of them? I am compelled to say they have not. Not one of them whose diameter exceeds 16 inches has been able to be employed in the study of double stars, nor, consequently, has exhibited in the heavens that separating power due to its diameter, and of which the perfection of its form should be the warrant. This is, from the point of view of the question we are discussing, a fact of the highest importance.

Let me make one remark here on L. Foucault's telescopes. I find that their illustrious maker has been wrong in wishing to tempt nature by giving too short a focus to his mirrors relatively to their diameter. No doubt it is very convenient to reduce to 23·6 feet the length of a telescope of 40 inches in diameter; Lassell, for the same diameter, gave 36 feet to his instrument. Without doubt the methods of testing the surfaces are sufficiently perfect to allow of our giving to these great mirrors that rigorously parabolic form which they ought to have. But it follows from this "*tour de force*" of Foucault, that his mirrors are only absolutely perfect along their axes, that the slightest distortion of their surface considerably disturbs the images, and that, above all, it is very difficult to use them either for spectroscopic observation, or for photographing the heavens. They are too perfect instruments, which are good only under the condition of preserving this perfection. I myself prefer a little more latitude in excellence. The English, in my opinion, have been better advised in preserving rather greater focal length?

Returning to large refractors. When the glass-founders were in a position to furnish crown and flint discs to opticians, all observatories wanted to obtain large equatorial refracting telescopes. The most celebrated, that of Pulkova with a 15-inch object-glass from the workshop of Merz of Munich, was regarded as the wonder of optical apparatus. Now, objectives of this size are numerous; Paris, Bordeaux, Nice, Lisbon, Madrid, Brussels, Rio-Janeiro, Dun-Echt, all have similar ones. But we have gone further still. Alvan Clark of Chicago, in 1862, made an object-glass of 18·5 inches aperture, and 23 feet focus, of which the fruit, and solitary fruit, was the discovery of the companion of Sirius. Mr. Newall, of Gateshead, near Newcastle, ordered an equatoreal of Cooke, of York, of which the objective was 24·8 inches in aperture, and 29·3 feet in focal length. But this fine instrument confined to the smoky sky of Newcastle, has, so they say, done nothing. Following this came, in 1872, the objective of the Naval Observatory at Washington, the work of Alvan Clark; it is 25·6 inches in diameter, and of 29·3 feet focus. Here at last we part from this class of gigantic instruments, which despite their optical perfection, have only been able to be employed in a very restricted kind of observations, and have never afforded the results which might, à priori, have been expected of them. Double stars, the systems of the satellites of Uranus and Neptune, and observations of nebulæ, the Washington equatoreal has been able to attack with success in the hands of MM. Newcomb, and Holden: lastly, the perseverance of Mr. Asaph Hall enabled him to discover, in 1877, the two satellites of Mars, that he had searched for in vain with telescopes of more than 11·8 inches in aperture. How is it that since this date this fine instrument has done nothing else, and that in his later publications, Mr. Asaph Hall has allowed a very marked discouragement to peep out, and has insisted on defects in the object-glass which appeared to have no existence at the beginning? This will be one of the most delicate points that we shall have to discuss.

The announcement of the important discoveries obtained by the aid of this instrument resuscitated the zeal of every observatory; each one wanted to have a big telescope, and a bigger one than its neighbour. The Vienna Observatory ordered an object-glass of 28-inches in aperture from Grubb, in Ireland. M. Otto Struve, at Pulkowa, got Alvan Clarke to make, with glass supplied by M. Feil, an objective of 31·5 inches. Finally, the observatory founded on Mount Hamilton, in California, by the munificence of Mr. Lick, will shortly be in possession of a glass of 38·5 inches in diameter. Paris also required her great object-glass. Foucault would have crowned his work by grinding Chance's great discs, in anticipation of which he had undertaken his optical researches, but he died when his work had just begun. M. Ad. Martin, the inheritor of his methods, which he has greatly improved, has continued his work; the flint found to be defective was replaced; the learned artist had brought the work nearly to an end when illness compelled him to abandon it.

Now we fear that the 28·7 inch objective will never be finished. We will console ourselves for this miscarriage by turning our eyes towards the shores at Nice, where the generous munificence of M. Bischoffsheim has founded and supported an observatory destined to compete with the most important foreign institutions, by the perfection of its equipment and the purity of the sky. I exhibit to you photographs representing the progress of the work for mounting this great telescope, at three different stages. The employment of such instruments demands—in the construction of the equatorial mounting which carries the telescope, of the dome which covers it, of the observing chair which brings the observer to the eyepiece in all positions of the telescope—the solution of problems which tax all the science and sagacity of the engineer. M. Struve said of his great Pulkowa telescope, which however was only 29·5 feet long, that such an instrument might well be called a *refractor*, since after he had broken his leg, it had broken it for him a second time. Thanks to MM. Ph. Gautier and Eiffel, the great equatoreal at Nice can be managed without either trouble or danger. The immense dome which covers it, a little larger than the dome of the Pantheon, notwithstanding its weight of 93½ tons, can be turned easily by one man, floating, as it does, in a circular trough filled with liquid. The closing of the shutters, so defective in ordinary domes, is here entirely water-tight. Finally, the observer on the seat which has to carry him to a height of 29·5 feet above the floor, will be protected against falls by a balustrade, which in no way hinders the movement of the telescope, and he can, without moving, place his instrument in any position, and verify its position at any instant. The instrument is now very nearly completely mounted. You will be unwilling to omit to wish good luck to the astronomers who during the coming summer are to explore the heavens by the help of this very powerful instrument.

(*To be continued.*)

LIVERPOOL ASTRONOMICAL SOCIETY.

The fifth annual general meeting for the election of officers was held on the 17th July, at the Association Hall, Mr. James Gill occupying the chair. The annual report on the state of the affairs of the society was read by Mr. R. C. Johnson, F.R.A.S., who acted as secretary *pro tem.* Since the last annual meeting there had been an increase of 21 members, making a total of 257. Twenty-eight papers had been read during the session, and published in the Society's *Journal.* More than a thousand observations of variable and red stars had been made by the Rev. T. E. Espin. The observations on variable stars had been communicated to Prof. Pickering, and published in the proceedings of the American academy of science and arts. A list of 109 new red and orange-red stars had also been published in the *Monthly Notices* of the R. A. S. Magnitudes of 524 stars had been deduced from photographs taken with the stellar camera, and were now ready for publication. The retiring president, Mr. Isaac Roberts, had also undertaken photographic work, and those who had examined the magnificent stellar photographs of the Brothers Henry, would understand the importance of this branch of the science. The instrument employed by Mr. Roberts was a reflecting telescope of short focus, and 20 inches diameter. Stars of even small magnitudes had been photographed very distinctly in 15 minutes; whereas the exposure recommended by Admiral Mouchez was from one to three hours. Much, however, depended upon the state of the atmosphere, and other conditions with which at present we were but little acquainted. As an example of this uncertainty, the companion to Polaris had been in one instance photographed in four seconds; whilst in another plate it did not appear after an exposure of 16 minutes. More than 200 photographs had been taken from the pole down to 69° decl. Amongst the difficulties to be enumerated were small specks upon the film which were hard to distinguish from stars even when viewed through a microscope.

The balloting papers were then opened by the scrutineers, and the following officers were declared duly elected:—President, Rev. T. E. Espin; Vice-Presidents, W. F. Denning, W. S. Franks, J. E. Gore, James Gill; Special Observer, Rev. T. E. Espin; Secretary and Treasurer, W. H. Davies; Council, Miss E. Brown, and Messrs. George Calver, W. H. Davies, junior, T. G. Elger, S. M. B. Gemmill, F. H. Masters, J. M. Offord, Rev. T. Perkins, John Stead, K. J. Tarrant, W. L. Stubbs, and H. Thomas.

A vote of thanks to the retiring officers was proposed by Mr. W. H. Davies, seconded by Mr. W. S. Franks, and carried unanimously, when the meeting adjourned till the 11th October.

SELENOGRAPHICAL NOTES, AUGUST, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Diophantus and Delisle. Though situated in a monotonous quarter of the Mare Imbrium, where there is little to break the uniformity of the grey tone of the surface, these ring-plains will repay observation if they are examined when the eastern longitude of the morning terminator ranges from about 39° to 42°. Diophantus, the smaller of the two formations, presents no features calling for special notice beyond a narrow gap or pass in its bright north border which is not shown in the maps. A short distance north-east of it, however, stands a very conspicuous crater, nearly as large as Diophantus *a*, which is omitted by Mädler and Neison, though inserted on Schmidt's Chart, *Sect.* V. For some hours after sunrise it is obscured by the prominent shadow thrown by the eastern wall of its larger neighbour, which probably accounts for its being frequently overlooked. On the west side of Diophantus *a*, lies a small, low, incomplete ring of a horseshoe shape and about equal to it in size. It resembles a crater whose border is partially destroyed. Schmidt merely draws a short straight ridge at this place, tangential to the rim of Diophantus *a*.

From the eastern side of Delisle and extending to a point about midway between it and Diophantus *a*, stretches a triangular-shaped mountain mass abruptly terminating on the south in a lofty peak. This object is connected with the east side of Diophantus by a very steep regular ridge or cliff which at sunrise and for some time after casts a shadow as sharp and uniform as that of the well-known fault near Birt. Curiously enough Schmidt, whose representation of this region is very exhaustive and true to nature, omits the prominent little crater which figures on the north wall of Delisle and shows a conspicuous peak in its place. This crater may easily be detected under a moderately low sun with small apertures. The light spindle-shaped marking extending from Diophantus *b* towards the north-east side of Diophantus is a noticeable feature in the neighbourhood, well shown by Schroeter (*Sel. Top. Frag.* T. XIX.) Between the broadest part of this object and the south wall of Delisle is a large circular white area, which at 8h. on 19th September, 1885, when the region was well placed for observation, was there estimated to be about 6° in brightness. Though very conspicuous, it does not figure in any map or description. About thirty miles due east of Delisle rises a rough triangular plateau, *a* (*alpha*) surmounted by three prominent peaks; and still further east lie some well-marked ridges, trending from the region west of Gruithuisen towards the Harbinger mountains.

The great Winding Valley by Herodotus. Between 7h. 30m. and 9h. on the evening of 17th March, I had an uninterrupted and favourable view of this remarkable formation with a power of 340 on my 8½ in. Calver-reflector. The rough mountain land east of Herodotus is traversed by a wide shallow valley, near the northern end of which lie two large bright mountains. The Herodotus cleft is first traceable on the west side of the larger of these objects debouching from the mouth of the valley. It is here very delicate, but as it progresses towards the north-west it gradually becomes wider and deeper, exhibiting in its course many irregularities, till it reaches its first sudden swerve to the south-west. In the broadest and deepest part of its course, about 30 miles north of Herodotus, the eastern slope alone of the valley is apparently nearly three times as wide as the bottom and western slope combined. In the widest part of this side I remarked a short branch cleft running from the upper surface down the slope and joining the deep chasm at a very acute angle directed towards the north. The details of the great valley between its wide club-shaped expansion north of Herodotus and the border of the ring-plain are always more or less difficult to observe satisfactorily. There appear to be two or three slight breaks or interruptions in the continuity of this section of it, the nature of which it is hard to determine. They are probably due to landslips from the mountain masses which border the western flank of the valley in this neighbourhood. In the late Prof. Phillips' drawing of Aristarchus and Herodotus in *Phil. Trans.*, and in that by Schmidt in his *Rillen,* the club-shaped widening of the valley is well-shown, but both drawings are on too small a scale to do justice to other details which a very moderate magnifying power brings to light. The coarse conspicuous valley or cleft which joins the great winding valley on the west, near its junction with Herodotus, and then winds partly round the west side of the ring-plain, is a feature which till lately escaped the notice of observers, a fact which shows how imperfectly even this most interesting region has been studied.

Kempston Beds, 20th July, 1886.

SIDEREAL NOTES FOR AUGUST, 1886.

By J. E. Gore, F.R.A.S.

The following known variable stars may be well observed in August:—

1. V Coronæ. R.A. 15h. 45m. 27s., N. 39° 55′ (1886·0). This is No. 361 of Birmingham's Catalogue of Red Stars. Its variability was discovered by Duner in 1878. The variation is from 7·5 to 8 mag. at maximum, to 12 mag. at minimum, and the period about 359·5 days. Chandler finds that the star is about 8½ mag. 50 days before, and 60 days after the maximum.

From elements given by Chandler I find that a maximum will be due on August 28.

2. U Herculis. R.A. 16h. 20m. 46s., N. 19° 9′ (1886·0). A variable discovered by Hencke in 1860. The variation is from 6·6 to 7·7 mag. at maximum, to 11·4—11·6 at minimum, and the period about 408 days. A maximum will be due this year on August 26. The variable may be easily found with a good binocular, as it lies about 1° *s.f.* γ Herculis.

3. R Draconis. R.A. 16h. 32m. 21s., N. 67° 3′ (1886·0). A variable discovered by Geelmuyden in 1876. It varies from 7·2 at maximum, to below 13 mag. at minimum, and the mean period is about 245 days. A maximum will be due this year about September 1.

A minimum of Algol may be observed on August 22 about 9 p.m.

Attention may again be directed to the new variables mentioned in the notes for June, as observations are very desirable.

The following "suspected" variables may be especially recommended for observation in August:—

1. Birmingham 372 Serpentis. R.A. 16h. 3m. 35s., N. 1° 8′ (1880·0). Rated 7 mag. by Lalande, Bessel, and Argelander. Birmingham's observations, 1874—1876, vary from 6 to 7·5. It is not in the *Uranometria Argentina.* It also seems to vary in colour. It was found at Dunsink to have no colour on April 30, 1880, and deep orange (!) on June 10. On September 20, 1884, I estimated it 6·7 in Gould's scale, and on April 28, 1886, 6·9 mag.

2. Lalande 29822. R.A. 16h. 16m. 55s., S 0° 35′. Rated 7 mag. by Lalande and Argelander, and 7·3 at Cordoba, but given by Heis as 6–7 mag. On September 21, 1884, I estimated it 7·3 in Gould's scale, and about 7·5 on April 28, 1886. The star may be readily found with a binocular, as it lies about 2° south of σ Serpentis, and is the southern of four small stars nearly in a straight line.

3. Birmingham 420 Ophiuchi. R.A. 17h. 48m. 8s., N. 1° 47′ (1880). Birmingham's observations, 1871—1876, vary from 7–8 to invisibility. He says: "It is certainly variable." Webb also suspected variation, and failed to see the star on August 7, 1871, and September 22, 1873. I found it invisible with the binocular, and therefore below 8½ mag. on the following dates:—May 2, 1878; September 20, 1884; August 15, 1885; and September 13, 1885.

4. Birmingham 422 Ophiuchi. R.A. 17h. 52m. 2s., N. 2° 44′ (1880). Birmingham's observations, 1872—1876, vary from 7 to 8·5 mag. On September 20, 1884, I estimated it

7½ mag.; August 15, 1885, below 8 mag.; and September 13, 1885, about 8 mag. Secchi found it golden yellow in July, 1868, "with traces of bright zones" in its spectrum, and near it "two other stars rose coloured." It may be easily found, as it lies about 40′ *s.p.* the 4 mag. star 67 (o) Ophiuchi.

5. Birmingham 487 Vulpeculæ. R.A. 19h. 3m. 37s, N. 23° 59′ (1880). Rated 6 and 7 by Lalande, and 7 mag. by Bessel. Birmingham's estimates vary from 6 to 7·5. Secchi found it "scarcely 8 mag., July 15, 1868." In August, 1869, he found it "5 mag. with uniform spectrum, bright in the yellow."

6. DM + 16°, 3936 Sagittæ. R.A. 19h. 34m. 0s., N. 16° 18′0″. A fine red star observed by Webb and Birmingham as 8·5 and 8·7 in September, 1877; but seen by Espin 7·5 mag. July 4, 1885, and 6·0 June 13, 1886. This star deserves careful observation.

7. Birmingham 551 Cygni. R.A. 20h. 12m. 38s., N. 39° 59.′5 (1880). Variable, according to Espin, from above 5·0 to below 7·0. It was 6·0 on October 1, 1885; 7·0 on November 30, 1885; and above 5 mag. June 13, 1886. The star is evidently variable, and requires careful attention. It lies a little *preceding* γ Cygni.

8. Birmingham 569 Delphini. R.A. 20h. 39m. 58s., N. 17° 39′ (1880). Found by Espin to be variable in 1884. My observations from September, 1884, to December, 1885, show a period of about 111 days ± with a minimum about December 28, 1885. Observations of this star are much required. If the above period is nearly correct a minimum will be due during the first half of August, 1886. The variation is from about 6·3 mag. to 7·5 mag. Duner finds a remarkable spectrum of Secchi's third type with extremely large and black bands, especially in the blue.

REVIEWS.

Eleventh Annual Report of the Savilian Professor of Astronomy to the Board of Visitors of the University Observatory. Read June 16, 1886.

Closely following the Visitation of the Royal Observatory comes that of the Oxford University Observatory. The two Institutions widely differ in one respect, for at the latter practical Instruction is given to outsiders, to whom lectures are offered. These have been attended during the past year by the average number of students; but, owing to the comparative difficulty and the considerable time requisite to master the more theoretical branches of astronomy, many are deterred from devoting much of their attention to such subjects.

Professor Pritchard purposes to turn his attention to stellar photography. For this purpose the De la Rue reflector will be employed.

After thirty years of constant work, it was found that some of the more delicate parts of the mounting, such as the slow motion in declination and right ascension, had become worn. Through Dr. De la Rue's liberality the necessary repairs have been, or soon will be, effected. The Professor says that he is "not without what at present appears to be a well-grounded hope that in the course of a year the parallax of certain stars will be re-determined by this new and fructiferous process, even to a greater degree of accuracy than has hitherto been achieved by direct instrumental application." We are told that the *Royal Society*—not the *Royal Astronomical Society*—has been the recipient of some considerable results in this new field of astronomy which have been already successfully obtained. It is a great pity that there is such a growing tendency to present papers on strictly astronomical subjects in the eastern, instead of the western, side of Burlington House.

Stellar photometry at Oxford has been brought to an honourable close—at any rate, for the present—by the publication of the *Uranometria Nova Oxoniensis*, and by the award to Prof. Pritchard of the Royal *Astronomical* Society's gold medal. By way of an appendix, he has directed "the measurement of the magnitude of an equatorial zone of stars, by means of which it will now be possible to connect the comparative brightness of stars in the southern hemisphere with those already determined in the northern, and also add to the convenience and accuracy of measures of magnitude of the fainter stars not yet observed in the latter." It is proposed to print these measures as a four-page supplement to the *Uranometria.*

The stars *Nova Andromedæ* and *Nova Orionis* have been carefully observed, and the results communicated to the Royal Astronomical Society.

The Professor's Report concludes with an honourable mention of the services rendered by the two assistants, Mr. Plummer and Mr. Jenkins.

A Treatise on Nautical Astronomy, for the use of Students. By J. Merrifield, LL.D., F.R.A.S., &c. London: Sampson Low & Co. 1886. Pp. 364.

This is a work written by a well-known teacher of nautical astronomy, who is, therefore, specially fitted for the task of bringing together all the facts of the science in such a manner as is best suited to meet the requirements of those who are preparing for examination on the subject. To this end the author has placed at the conclusion of each chapter a large number of very useful questions (669 in all) gathered from the papers set by the Admiralty, the various Universities, the Science and Art Department, and other sources. The student is thereby enabled to judge for himself of the nature of the examination for which he intends to offer himself. The work, however, by no means partakes of the nature of a cram-book. It is intended as a companion to the author's *Treatise on Navigation*, which explains its somewhat awkward commencement, "The *second* method of finding a ship's place." In all sciences it is of the highest importance that we should have the necessary definitions at our fingers' ends, and the author very rightly devotes several pages to explanations of the different terms employed. These are models of clearness of expression. Many problems are given which appertain to the domain of physical—rather than of nautical—astronomy; and for the full understanding of some of these a knowledge of spherical trigonometry is required. It is not assumed that the reader is at all acquainted with the subject before his introduction to the book; every fact is therefore clearly explained, and the student who thoroughly masters each chapter in succession, and carefully works out the problems, will, by the time he arrives at the end of the book, be a master of the science, and quite able to grapple successfully with any problem that may be put before him in nautical astronomy. A little more care on the part of the engraver would have caused Fig. 7 to be accurate. Fig. 8 is altogether wanting. We would suggest that in a second edition an index might be added with advantage, and a few misprints corrected.

Twentieth Report of the Board of Visitors to the Victoria Observatory, together with the Annual Report of the Government Astronomer. 1885.

The latter part of this publication, the Report of the Government Astronomer, is the more important, though it is satisfactory to note that the Board of Visitors are highly pleased with all that Mr. Ellery has been, and still is, doing for the good of astronomy and meteorology.

The report is brought down to June 30, 1885. The great telescope has continued to do satisfactory work, but the dropping of the north pivot increased so much in the autumn that it became necessary to adjust it without further delay. This was effected by means of screw-jacks, and the whole instrument, after a thorough overhaul and cleaning, was re-adjusted throughout. It may now be regarded as in excellent working order. Although the brilliancy of the mirrors has considerably diminished, its capacity for work is not sensibly lessened. Much trouble was caused by the very heavy dews leaving films on the large mirror, which would not always dry off, even in full sunlight. The new transit circle gives great satisfaction for facility in observing, excellence of performance, and good results. The instrument throughout reflects great credit on the makers, Messrs. Troughton and Simms. The determination of the errors of pivots and circles is at once to be entered upon; these, whatever they may be, the Government Astronomer says, the results show must certainly be very small. As it is necessary, during observations with the transit-circle, to take frequent thermometer readings in the open air, a thermometer stand has been erected about 50 feet from the building, and arrangements have been made for reading the scale from inside the transit-room by means of a telescope fixed in the south door. The necessary illumination of the scale at night is effected by a small incandescent lamp, the proper precautions being taken to prevent the thermometer being affected by radiant heat from the lamp. The 8-inch and the 4½-inch equatorials continue in capital working order. An alteration has been made in the photo-heliograph by which pictures of eight inches, instead of four, will in future be taken. As the Solar Physics Committee have undertaken the measurement of all sun-pictures, the whole of the stock, consisting of 1,712 photographs on glass, was sent to London in March, 1885. It is satisfactory to learn that, out of this very large number, only 13 were found on their arrival to be broken. One or two modifications have been made to the new barrel chronograph used with the transit-circle, the chief of which is the adoption of what appears to be a form of Sir W. Thomson's syphon recorder, in place of the ruling pen. The first part of the results of the work with the great telescope from its erection, in 1869, to the date of the report, has been published. The satisfactory reproduction of the drawings of nebulæ still remains a difficulty, and Mr. Ellery gives as his opinion that, although the lithographs of the 49 nebulæ described in that publication are fairly successful, they cannot be considered all that could be desired. The work done with the great telescope has been chiefly confined to a revision, by Mr. Baracchi, of southern nebulæ, already observed by former observers, preliminary to publication. One hundred and seventy-two nebulæ have been re-observed and re-drawn for this purpose ; η Argus and 30 Doradus have also been revised. Of the total number 29 are new, or not identified, found by Mr. Baracchi. There are still 240 nebulæ observed by former observers which have yet to be finally revised before publication.

The number of observations made with the transit-circle have been as under :—

Right Ascension observations	2,287
Polar Distance „	983
For Collimation error	188
„ Level „	221
„ Nadir „	200
„ Runs of Microscopes error	50
„ Flexure of Telescope „	13

The new catalogue will contain the meridian results from 1871 to 1884 inclusive.

In extra-meridional work some experiments have been made with Prof. Pritchard's method of star photometry. The observations respecting meteorology and terrestrial magnetism have " gone on much as usual," but no details are given in the report. The Government Astronomer purposes to make a trial of a new form of pendulum seismometer, which has been adopted by Prof. Milne, of Tokio ; at the same time he intends to continue the revision of the nebulæ, and all the routine meteorological, magnetic, and other physical work.

We have received *Sirius* for April, which contains—besides articles on large double telescopes, on the Light of the Stars, particularly with reference to the *Nova Andromedæ*, and other minor papers—a reproduction of the photograph of the *Pleiades*, taken by the Brothers Henry.

The May number has articles on the great meteor shower of November 27, 1885, by Professor Schiaparelli, on a new determination of the radiant heat of the sun, and on the nebula near Maia.

We have received also Nos. 2,721 and 2,722 of the *Astronomische Nachrichten.* The former contains, *inter alia,* Observations of the occultations and eclipses of the satellites of Jupiter, made at Strasburg, Plousk, and Rio de Janeiro. To the latter M. Jesse contributes an article on the " Determination of the heights of shooting stars." It contains also elements of Comet 1885, II., and elements and an ephemeris, for the latter half of April, of Comet *Fabry.*

Professor Asaph Hall says, in a note which appears in No. 2723 that in a photographic print of the Pleiades, furnished him by Professor Pickering, he noticed a marked blurring on the preceding side of Maia, which he attributed to a defect in the print. The blur, however, appears on all the prints, and is doubtless the nebula discovered by Messrs. Henry. With the 26-inch refractor, although there is nothing visible round Maia which would be taken for a nebula, a bluish halo precedes the star, similar to that surrounding Alcyone. This appearance may be due to the correction of the object-glass. Professor von Schur communicates the results of his observations for the determination of the parallax of the double star ψ^5 Aurigæ, whose place for 1885 is R.A. 6h. 38m., Decl. 43° 42′ N. He gives as its parallax $0''\cdot111 \pm 0''\cdot034$.

No. 2726 of the same publication contains a view of the nebula in the Pleiades, in illustration of a paper on the same by Professor Weiss. M. Laschober gives the places of the new stars in Andromeda and Orion.

The Imperial Observatory of Rio de Janeiro, which contains several first-class instruments, now issues a monthly Report. No. 3 of the *Revista do Observatorio* for March last contains short articles on the relation between the velocities of parabolic and circular orbits; equatorial currents in the atmospheres of the sun and planets; the barometric pressure compared with the temperature at Rio; waves of light, heat, and sound. The aspect of the heavens (accompanied by a plate) for the month of April is very fully described, and a list is given of the phenomena of Jupiter's satellites. It will be remembered that a full description of the Observatory was given in our last volume.

In No. 4 we have an account of the labours of the Brothers Henry with respect to stellar photography, accompanied by an illustration of the new double telescope recently made and equatorially mounted at the Paris Observatory for that purpose. The following has been found to be the minimum time of exposure necessary, making use of the gelatino-bromide plates of Monckhoven :—

Magnitude.	*Time required.* h.	m.	s.
1	0	0	0·005
2	0	0	0·013
3	0	0	0·03
4	0	0	0·08
5	0	0	0·2
6	0	0	0·5
7	0	0	1·3
8	0	0	3
9	0	0	8
10	0	0	20
11	0	0	50
12	0	2	0
13	0	5	0
14	0	13	0
15 16	1	23	0

The relative time required for obtaining a photograph of a star of the first magnitude compared with that needed for a picture of one of the fifteenth or sixteenth is, therefore, 1 : 1000000.

Observations of Variable Stars in 1885. By Professor Pickering. (Reprinted from the *Proceedings of the American Academy of Arts and Sciences*).

In a paper communicated last February to the American Academy particulars are given of nearly 200 variable stars observed in 1885. Considering the comparative ease with which observations of this class are made, it is a wonder that, at a time when there are so many star-gazers, more do not apply themselves to this interesting work. But little is wanted beyond a knowledge of the heavens, and some slight instrumental means. Indeed, many of the observations recorded have been made by means of field-glasses, notably those to which the names of Mr. Gore, and of Mr. Sawyer, of Cambridge Port, Massachussetts, are appended. On the other hand, the telescopes mentioned as having been employed vary in aperture from 2 inches to Mr. Espin's 17¼-inch reflector. The present publication is the third in a series of annual statements relating to variable stars, which was begun in 1884. All who are willing to assist in the work, which, it is hoped, will result in the publication of a new Catalogue of variable stars, are requested to send accounts of their work to the Harvard College Observatory as soon as possible after the close of each year. The principal facts desired are the designations of the stars observed, and the number of nights on which each of them was examined. Brief statements of the results obtained, such as the maxima and minima, are also desirable, as well as information with regard to the method of observation, and the instruments employed.

Atmospheric Refraction. By Professor Pickering. (Reprinted from the *Proceedings of the American Academy of Arts and Sciences.*)

During the summer of 1876 several thousand observations of the altitude of the White Mountains were made by the writer. The method of zenith distances was employed, the instrument used being the micrometer level. Great difference of opinion exists regarding the value of the zenith distance of a distant object as a means of determining its height, on account of the uncertainty in the atmospheric refraction. A large number of measures of this quantity were accordingly made in subsequent years to show to what extent the observations of 1876 were likely to be affected by this error. The various forms of micrometer levels employed are described, and the results of a considerable number of observations exhibited in a tabular form. The instruments were also used to determine the effect of refraction as regards astronomical observations. The study of this matter has usually been left to the large altazimuths and transit-circles of astronomical observatories. From the fixed position of these instruments it is not easy to vary the conditions as much as might be desired. The variations of refraction in different parts of the world, or in different azimuths, or the effects upon it of the proximity of large masses of water, forests, or snow-covered mountains, are, therefore, but little known; and the refraction tables of Bessel, or of the Pulkova Observatory, are usually employed under the most varied conditions. Professor Pickering shows conclusively that the micrometer level is capable of giving useful results where a larger instrument has generally been considered necessary.

Traité de la Détermination des Orbites des Comètes et des Planètes, par T. d'Oppolzer. Edition Française. Par Ernest Pasquier. 1er volume Paris: Gauthiers-Villars. 1886. Pp. xxvi. 491, and tabular matter ccix.

M. Pasquier deserves the thanks of the rapidly decreasing number of astronomers who are not acquainted with the German language, for having translated this great and already classic work of M. Oppolzer, who has been engaged for upwards of a quarter of a century in the determination of these orbits. The work will commend itself to all who are able to bring to bear on it an acquaintance with the first notions of the differential and integral calculus. The first edition of the original treatise appeared in 1869; the second—of which this is a translation—in 1882. It is conspicuous for the clearness of its style, and the elegance of its formulæ. In the choice of methods, M. Oppolzer has selected those which most surely lead to the desired end, and, whilst giving most rigorous solutions, has supplied others, less lengthy, but only approximate. Frequently the same question is resolved in two ways. The volume under consideration comprises two parts, the first of which is devoted to the establishment of a certain number of fundamental relations, such as the corrections for parallax, aberration, precession, and nutation, Euler's equation, and the change in it proposed by Encke, &c. The second part contains the actual determination of the orbits by means of three or four observations. To assist in working out the problems detailed examples are given, showing how to determine the orbits of several comets and minor planets. In an appendix the author has brought together a collection of the necessary formulæ. It is almost needless to say that the whole work bristles with figures. As a most valuable contribution to science we have very great pleasure in commending it to the notice of every physical astronomer.

The following are the elements of Brooks' first Comet, as computed by Professor Holebschek, from observations at Vienna and Rome on April 29, Hamburg on May 1, and Arcetri on May 3:—

$$T = 1886,\ \text{June } 6{\cdot}4951\ \text{M.T. Berlin.}$$

$$\begin{array}{r} \omega = \\ \Omega = \\ i = \end{array}\left.\begin{array}{rrr} 204^\circ & 40' & 13'' \\ 190 & 47 & 8 \\ 87 & 20 & 30 \end{array}\right\}\text{Mean Equinox } 1886{\cdot}0.$$

$$\log q = 9{\cdot}38219$$

Astron. Nach. No. 2729.

Professor Spitaler has computed the following elements of Brooks' second Comet from observations at Kiel on May 2; Hamburg, May 4 and 5; and Vienna, May 7:—

$$T = 1886,\ \text{May } 4{\cdot}25956\ \text{M.T. Berlin.}$$

$$\begin{array}{r} \omega = \\ \Omega = \\ i = \end{array}\left.\begin{array}{rrr} 38^\circ & 4' & 22''{\cdot}2 \\ 287 & 23 & 37{\cdot}4 \\ 99 & 50 & 29{\cdot}3 \end{array}\right\}\ \text{Mean Equinox } 1886{\cdot}0$$

$$\log q = 9{\cdot}925072$$

Astron. Nach. No. 2729.

EPHEMERIS OF COMET FABRY.

At 12 0 0 M.T. Berlin.

	α h.	m.	s.	δ °	′	log. r	log. Δ	H.
August 1	10	9	36	—48	45·3	0·3378	0·3659	0·5
2		10	57	48	56·2			
3		12	18	49	7·2			
4		13	39	49	18·3			
5		15	0	49	29·5			
6		16	21	49	40·7	0·3514	0·3839	0·4
7		17	42	49	52·1			
8		19	2	50	3·5			
9		20	23	50	15·0			
10		21	44	50	26·6			
11		23	5	50	38·3	0·3644	0·4006	0·4
12		24	26	50	50·0			
13		25	47	51	1·9			
14		27	8	51	13·8			
15		28	28	51	25·9			
16	10	29	49	—51	38·0	0·3769	0·4164	0·3

A. SVEDSTRUP.

EPHEMERIS OF COMET BARNARD.

At 12 0 0 M.T. Berlin.

		α		δ		log. r	log. Δ	H.
	h.	m.	s.	°	′			
August 2	10	8	25	—57	12·5			
4		12	28	57	29·9	0·2781	0·2707	1·7
6		16	27	57	47·7			
8		20	23	58	5·8	0·2918	0·2905	1·5
10		24	16	58	24·3			
12		28	7	58	43·2	0·3049	0·3091	1·3
14		31	55	59	2·4			
16		35	42	59	22·0	0·3176	0·3267	1·1
18		39	27	55	42·0			
20	10	43	10	—60	2·3	0·3297	0·3433	1·0

J. v. HEPPERGER.

Astron. Nach. No. 2723.

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Aug. 1	+66	55M.	Aug. 11	—55	15M.	Aug. 21	+ 2	36 E.
2	54	42	12	67	28	22	— 9	36
3	42	29	13	79	41	23	21	49
4	30	16	14	+88	6 E.	24	34	1
5	18	3	15	75	54	25	46	14
6	5	50	16	63	41	26	58	27
7	— 6	23	17	51	28	27	70	39
8	18	36	18	39	16	28	82	52
9	30	49	19	27	1	29	+84	56M.
10	43	3	20	14	49	30	72	43
						31	60	30

Moon farthest from Earth, Aug. 15, 2h.
,, nearest to ,, ,, 28, 21h.

COMET-METEOR RADIANTS.

	Designation of Comet and Discoverer.	Earth's Passage through plane of Comet's Orbit.	Comet-Meteor Radiant for the day of Earth's Passage. R.A.	Dec.
1885 II.	Barnard	June 23	325°·8	+55°·0
1885 III.	Brooks	April 14	184 ·5	—73 ·9
1885 V.	Brooks	June 13	201 ·4	—87 ·0
1886	Fabry	April 26	322 ·6	+36 ·9
1886	Barnard	May 29	350 ·8	+49 ·0

Sidereal Messenger, May, 1886.

ASTRONOMICAL OCCURRENCES FOR AUGUST, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Cygni. —
Sun	1	1	Conjunction of Moon and Mercury 3° 5′ S. Sidereal Time at Mean Noon 8h. 40m. 4·29s.	2nd Ec. R.	9 0 5	11 55·6
Mon	2		Sun's Meridian Passage 6m. 1·06s. after Mean Noon			11 51·6
Tues	3	9	Conjunction of Moon and Jupiter 1° 8′ S.	1st Tr. I. 1st Sh. I.	7 55 8 55	11 47·7
Wed	4	19 55 11	☽ Moon's First Quarter Conjunction of Moon and Mars 3° 57′ S.	1st Ec. R.	8 16 16	11 43·8
Thur	5	9 54 10 53 10 3	Occultation of 95 Virginis (6) Reappearance of ditto Near approach of 94 Virginis (6)			11 39·8
Fri	6					11 35·9
Sat	7					11 31·9
Sun	8	7 15 8 34 2	Occultation of 24 Scorpii (5) Reappearance of ditto Conjunction of Saturn and Venus 0° 1′ S.			11 28·0
Mon	9					11 24·1
Tues	10					11 20·2
Wed	11	6 44 8 0	Occultation of *d* Sagittarii (5) Reappearance of ditto Saturn's Ring: Major axis=37″·97 Minor axis=15″·49	3rd Sh. I.	7 39	11 16·2
Thur	12	22 50	○ Full Moon	1st Sh. E.	7 33	11 12·3
Fri	13					11 8·4
Sat	14		Sidereal Time at Mean Noon 9h. 31m. 19·50s.			11 4·4
Sun	15	20	Inferior conjunction of Mercury and Sun Illuminated portion of disc of Venus=0·878 Illuminated portion of disc of Mars=0·901			11 0·5

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	16	7 6	Conjunction of Uranus and Jupiter 0° 31′ N. Sun's Meridian Passage 4m. 4·60s. after Mean Noon			α Cygni. — 10 56·6
Tues	17	9 58 10 3 10 32 10 11 11 21	Near approach of 4 Ceti (6) Occultation of 5 Ceti (6) Reappearance of ditto Occultation of B.A.C. 5 (6) Reappearance of ditto	**1st Sh. E.**	8 26	10 52·6
Wed	18			**3rd Tr. I.**	8 11	10 48·7
Thur	19	10 42 11 47	Occultation of ν Piscium (4½) Reappearance of ditto	**1st Sh. I.**	7 13	10 44·8
Fri	20	17 55 12	☾ Moon's Last Quarter Neptune at quadrature with the Sun			10 40·8
Sat	21					10 36·9
Sun	22	13 23 13 54 15 22 16 13 16 1	Occultation of 48 Tauri (6) Reappearance of ditto Occultation of γ Tauri (4) Reappearance of ditto Near approach of 58 Tauri (6)			10 32·9
Mon	23					10 28·0
Tues	24					10 25·1
Wed	25	22	Conjunction of Moon and Saturn 3° 39′ N.			10 21·2
Thur	26					10 17·2
Fri	27	9 18 8 21	● New Moon Conjunction of Moon and Venus 3° 0′ N. Conjunction of Moon and Mercury 0° 14′ N.			10 13·3
Sat	28					10 9·4
Sun	29		Eclipse of the Sun invisible at Greenwich			10 5·4
Mon	30					10 1·5
Tues	31	3	Conjunction of Moon and Jupiter 1° 51′ S. Saturn's Ring: Major axis=38″·81 Minor axis=15″·50			9 57·6

THE PLANETS FOR AUGUST.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	10 6 14	N. 7 52	9″·8	1 25·9
	9th	9 56 32	N. 7 31	10″·8	0 44·8
	17th	9 30 26	N.10 20	10″·6	23 43·4
	25th	9 19 57	N.13 25	8″·8	23 1·5
Venus ...	1st	6 39 16	N.22 27	12″·2	21 55·6
	9th	7 20 39	N.21 51	11″·8	22 5·4
	17th	8 1 46	N.20 35½	11″·4	22 15·0
	25th	8 42 16	N.18 42	11″·2	22 23·9
Mars ...	1st	13 3 33	S. 6 59½	7″·0	4 22·8
	13th	13 30 42	S. 9 53½	6″·8	4 2·6

Mercury is badly situated for observation, setting soon after the sun at the beginning of the month. Towards the middle of the month he rises a few minutes before the sun, the interval increasing to about an hour and a half by the last day.

Venus rises an hour and three quarters after midnight on the 1st, the interval increasing.

Mars sets about two hours before midnight at the beginning of the month, the interval increasing.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 285. SEPTEMBER. 1886.

SPECTRUM ANALYSIS.[*]

The Astronomer-Royal for Scotland, who has devoted so much time and labour to spectrum observations, published some five or six years ago a paper on the general appearance of Gaseous spectra, as seen on a very small scale. But he intended this to be only the forerunner of a more extensive work, which should contain very highly dispersed, and much magnified, views of the more interesting and probably crucial portions of the most important of those spectra. In the *Upsala Transactions* for 1875, MM. Angström and Thalén, had treated of the spectra of a few gases, and of certain small portions had given largely magnified representations. The dispersion power employed by these eminent physicists appears, however, to have been insufficient to bring out many of the exquisite details which the spectroscopy of the present day demands for every gas that is observed. Prof. Smyth used for his present work a spectroscope giving a dispersion of 60° from A to H, as against 3° in his former instrument, and 24° in that of Angström and Thalén. By its aid so magnified a representation has been obtained that the whole visible spectrum would, on the same scale, extend to 220 feet in length. The world, however, we are told, is not yet ready for such a scale, consequently the original record has been reduced on subsequent working sheets to a 40 foot spectrum length, and reproduced by means of photo-lithography, light being shown by black, and darkness by white. The first spectrum treated of is what the British Association calls the "Candle-spectrum, in air," or, as named by Prof. Smyth, CH, *i.e.*, Carbo-hydrogen, in blow-pipe flame. In this spectrum there are five distinct bands, the first of which is orange, the next citron, and the others are, respectively, in the green, blue, and violet regions. Viewed in an ordinary single prism spectroscope, each of the first four of these bands begins on the red side with a strong line, followed by two, three, or more similar lines, but in decreasing brightness and lessening distance, interspersed with haze; while the fifth band seems to be composed of nothing but haze. More powerful spectroscopes have occasionally shown this luminous hazy mist to be more or less resolvable into faint and exceedingly close lines or linelets. The great dispersive power of Smyth's instrument not only shows it all so resolved, but it has enabled him to measure almost every linelet by micrometer, until they become too faint to be distinguished in any manner whatever. They are measured in "wave-numbers," not "wave-lengths." On this scale the A line in the solar spectrum corresponds to 33,400, the D^1 line to 43,108, and the H line to 64,012. These five bands were found to contain more than 400 lines and linelets, sufficiently distinct to admit of measurement, viz., 81 in the orange band, 94 in the citron, 97 in the green, 107 in the blue, and 71 in the violet, besides, in all probability, a large number of others, too faint to be measured. There seemed to be some law of increasing distance between the linelets, but of decreasing distance between the leading lines of each band, in proceeding from the red to the violet end. When a high temperature was required, an induction coil capable of giving a spark of from two to four inches in air, in connection with a bichromate battery, was used. To illustrate the effects produced by different methods, the D^1 and D^2 lines are shown as seen in a Bunsen-burner gas flame, by simple induction spark, by jar discharge or condensed spark, and in a vacuum tube; the range extends from broad haziness in the first to sharp, solid lines in an absolutely black field in the last. The tube incandescence is, therefore, the most favourable for accurate micrometrical measurements. The British Association, in 1880, accused Prof. Smyth of not filling his own tubes, but trusting to the labels put on them by the glass-blowers, which are sometimes fallacious. To meet this objection we are told who filled the tubes, and what stringent precautions were adopted during that important operation, as well as afterwards, to guard against error.

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[*] *Micrometrical Measures of Gaseous Spectra under High Dispersion*, by C. Piazzi Smyth, F.R.S.E., &c. *The Visual Grating, and Glass-lens, Solar Spectrum in* 1884. By the same. Reprinted from the Transactions of the Royal Society of Edinburgh.

Prof. Smyth devoted his attention chiefly to the spectra of coal gas and olefiant gas. After discussing the different bands at some length, we come to the summing up of the whole matter, —that the "London magnates," *i.e.* the Royal Society and the British Association, have been forced to come to the unpleasant conclusion that they are not always in the right, and that, after all, the candle-spectrum is a CH, not a C, spectrum. Dr. Attfield's conclusions, enunciated in 1862, have had to give way before the published memoirs of MM. Angström and Thalén, and Professors Liveing and Dewar, the opinion of all of whom on this matter had long been anticipated by Professors Smyth and Swan. The observations of the CO spectrum led to a most important discovery, that of the law previously alluded to. In Smyth's plates the tiny "peninsula" of pure green CO illumination showed no less than 44 lines. One of the original records of this part of the spectrum was sent to Prof. A. S. Herschel, who showed that each of these 44 lines was a necessary step in a remarkable system of physical numeration, proceeding in two rows of simple arithmetical progression, one over, but slightly advanced upon, the other. On this subject—as the matter cannot be well condensed—the reader is referred to Prof. Herschel's letter (accompanied by an explanatory plate) printed in *Gaseous Spectra,* p. 454. In the matter of the low temperature oxygen spectrum our author makes out another case against the Royal Society. This body printed a paper in 1879, showing that in the O spectrum there were four lines and four only, though the map which accompanied the paper shows 8, whereas Smyth, by using vacuum tubes end-on, in 1879, discovered many more. This important paper is accompanied by 31 plates, some of which are of very large size.

The occurrence of the after-glows, which were so conspicuous nearly three years ago, and the possession of one of Prof. Rowland's diffraction gratings, ruled at the rate of 14,438 lines to the inch, induced Prof. Smyth to endeavour to make a map of the solar spectrum on a large scale in 1884. He determined, if he could observe one spectrum well, to follow it up by a second, and even a third, time of going through the whole of it; with the view of eventually bringing these three records together in such a manner as to facilitate their comparison, and the result is contained in the second of the two quarto volumes now under consideration. For various reasons the neighbourhood of Winchester was considered likely to be a favourable place for such an undertaking. Accordingly, at "Kurn Hattin," a country house about two miles north of Winchester, he set up his apparatus, consisting of the grating which had been adapted for the purpose by Messrs. Cooke and Sons, and a heliostat. The instrument to which the grating was applied had object glasses of 4 inches in diameter, employed a magnifying power of 67, and was specially adapted for securing differentially, but with remarkable rapidity, a highly magnified record of the whole visible solar spectrum. The prism was of moderately good flint glass of 38° refracting angle, and measuring 5 × 5 inches. It was inserted transversely into the cone of solar rays coming from the heliostat lens, 65 inches before they arrived at a focus, and the large spectroscope table, carrying collimator, grating and telescope, rotated horizontally round a vertical axis under the centre of the prism. It could afterwards be slowly adjusted or definitely fixed by a tangential screw motion, when the collimator had been placed by trial in the line of minimum deviation for the prism at the part of the spectrum under observation with the grating. During the two months of observations, June and July, his wife, his painstaking and skilful assistant on former occasions, was laid up with a severe illness: he, however, was fortunate enough to secure the assistance of Col. Knight, whose residence and observatory were a short distance off. The three spectra obtained are reproduced, on a reduced scale of one third, in 60 plates, each of which contains, in addition, a part of Kirchhoff's Solar Spectrum from W.L. 7,690 to 7,365 after which it is replaced by Angström's Normal Solar Spectrum extending to W.L. 4,031, (these correspond, respectively, to wave-numbers 33,000, 34,500, and 63,000,) the spectrum obtained in 1882 by M. Fievez, for which are occasionally substituted those of MM. Cornu, Thollon, Vogel, and Profs. Young and Rowland, and scales of wave-lengths and wave-numbers. In the reduction of the original drawings each of the spectra was treated in perfect independence of the others—more than this, several instrumental changes were made before observing the third. A certain amount of symbolism is adopted in the illustrations, *e.g.*, the vanishing into darkness of the continuous spectral light, which forms the luminous background of the spectrum except at its extremities, is represented by a black shade running along the lower side of the strip, and gradually rising in it as the spectral light fails. The general rules adopted by Prof. Smyth, which obviate the necessity of any description in words, are (1) a vertical line stands for an actually measured Spectroscopic line; (2) lines in any other direction denote nebulous shade; (3) greater or less height of lines or of shaded bands denotes their greater or less visibility; (4) cones of shade indicate nebulous bands very gradually shaded off towards either side. When a comparison of the three Winchester spectra was instituted, some discrepancies were found in (*a*) the exact places and appearances of strong and well-known lines, and (*b*) the existence or non-existence, as well as the exact places, of very thin lines not hitherto generally known or recognised among most observers. Some of these differences are referred to the effects of varying temperature on the grating, while for others several causes are suggested. The general result, which Prof. Smyth arrived at from the whole of his observations, was the undeniable indications in the Winchester spectra of the upper air being at the time of observation overcharged with opaque, dusty particles, which might be due to the Krakatoa explosion or some other source.

THE PART TAKEN BY LARGE INSTRUMENTS IN ASTRONOMICAL OBSERVATIONS.

A Lecture delivered at the Sorbonne on March 6th.

By M. Wolf, Member of the Institute.

(*Translation.*)

(Continued from page 205.)

Let us now sum up, in a few words, the results and the discoveries which have been obtained by means of large instruments, and compare them with those which refractors and reflectors of ordinary dimensions have given. Great telescopes—I mean those of which the mirror is at least 19·7 inches in diameter—have never been of any use for the observation of double stars, consequently they would not seem to possess in practice the separating power assigned to them by theory and the perfection of their form. But the enormous quantity of light which they grasp has rendered them extremely useful for the observation of the satellites of planets when they are not too near to their primaries, and, above all, for observing nebulæ. A reflector of 7·9 inches, and even one of 14·8 inches, failed to enable M. Stephan, at Marseilles, to see the small nebulæ which he discovered by hundreds in the sky, by help of an instrument of 31·5 inches aperture. But for all that, when the object is sufficiently bright, even the details of a nebula are as well shown in a reflector of 15·7 inches, as in a larger ones, as I have just now shown you, in comparing the drawing of the nebula in Canes Venatici, made by Chacornac, with those obtained by the 47·2-inch Paris reflector, or that at Birr Castle. I have observed a great deal with two instruments of 15·7 inches and of 47·2 inches. I have rarely found any advantage in using the larger one when the object was sufficiently luminous; but we must not conclude, with certain

English observers, that small instruments are as valuable as the largest ones; it needs solely to fix a limit beyond which the advantage of the size of the mirror becomes most frequently illusory. I have always known a telescope of 15·7 inches aperture surpass one of 7·9 inches; thus in the Mare Serenitatis, in the moon, I have been able, by the aid of a 15·7-inch mirror, to ascertain the existence of numerous minute craters, absolutely invisible in a smaller instrument. This seems to me the most advantageous size for a mirror for general use.

Refracting telescopes, compared among themselves, lead us to a similar conclusion. The solitary important discoveries due to instruments with an aperture exceeding 15·7 inches are that of the satellite of Sirius, and those of the satellites of Mars; but the satellite of Sirius and the satellites of Mars are readily observable with objectives of 9·4 inches aperture and less. Doubtless it is one thing to observe an object whose position is known, and another to discover it. The astronomer can measure the position of a star which he perceives, with difficulty, and by fits and starts, and does not hesitate to publish his observations when he knows that it really exists in the place where he believes he sees it; while he does not dare to publish a discovery founded solely upon a very uncertain glimpse, and lets an important one escape which he would certainly have made with a more powerful instrument. Nevertheless, it is admitted by all astronomers that for ordinary use an objective of 15 inches diameter will show everything in the heavens that can be discovered by instruments of larger aperture, and the telescope is much more manageable than the colossi which are now constructed. This is the true instrument for daily observation, like the reflector of 15·7 inches.

If now we compare reflectors with refractors, we shall find that the advantage nearly always rests with the latter. A refractor, even a small one, surpasses, and, moreover, always seems to surpass, even a larger reflector. The lunar and planetary detail is generally better seen in a small refractor than in a great reflector. This is a point quite beyond doubt, and it is this upon which English astronomers have chiefly founded their argument against the use of large instruments. I have often made the experiment in the observatory. When people accustomed to physical and geodesical observations have viewed the moon through the fine 15·7-inch reflector made by Foucault, and through a refractor, also excellent, but of only 6·3 inches in aperture, I have always heard them proclaim the superiority of the smaller instrument. Indeed, the image of the moon, in a refractor, exhibits an aspect much more agreeable to the eye, a sort of "velvety" one—if I may so speak—which has no existence in the cold and sharp image given by the mirror of a reflector, and the details seem to come out better in the first than in the second. It is a curious thing, this actual quality of the images in the refractor is due to a defect in the objective, which is not absolutely achromatic; it fringes the outlines of the most brilliant parts, and this coloured fringe, projected on the dark details in the neighbourhood, makes them come out better. The reflector gives an image in black and white; the refractor illuminates this image.

In these terms, then, I state the conclusions which seem to me to be derivable from experience :—

1. An instrument of the largest aperture is always more valuable than a small one, for whatever class of observation it is employed, double star, planetary detail, or nebula, as long as it does not exceed the limit of size which I have fixed commonly at 15·7 inches for reflectors as well as for refractors.

2. With equal apertures the refractor surpasses the reflector.

Is there anything in these conclusions which is contrary to the theory of optical instruments soundly interpreted? This we must now examine.

Theory teaches us to distinguish those qualities of an objective or a mirror supposed to be perfect. Moreover, as far as its form goes, they are the light-grasping power, the power of penetration, and the optical power, or power of separation. The quantity of light emanating from a luminous point, that an astronomical instrument condenses at its focus in the image of this point is proportional to the area of the mirror or objective, and hence, as the square of its diameter. If, consequently, the naked eye, by the opening given to the pupil, ceases to perceive a star of less than the 6th magnitude, a telescope of double or triple the aperture of that of the pupil will enable us to see stars four times, nine times as faint as the 6th magnitude. It follows that if a 6th magnitude star were transported to twice the distance, when it would disappear to the naked eye, it would suffice to use an object-glass or a mirror of double the diameter of the pupil to render it visible. The power of penetration in space, then, is in proportion to the diameter of the objective. Finally, the dimensions of the image of a point—of a star—become less as the object-glass or the mirror becomes greater; two neighbouring points, two stars forming a pair, two points in contact on the disc of a planet, will be seen more widely separated as the diameter of the objective or the mirror increases. The separating power will be in proportion to its diameter.

Thus, according to theory, an objective or mirror will be more powerful, will enable us to see fainter objects farther off, will better separate the two components of a double star, will enable us to perceive more delicate detail on the disc of a planet in proportion as their diameter increases. The only limit to their power is that imposed by the impossibility of constructing perfect lenses and mirrors of dimensions superior to that limit. We must not pause in the construction of larger and longer instruments until the glass-founder says to us, I can make no larger glass discs; until the optician declares that he cannot grind greater ones; until the engineer is impotent to construct mechanism to carry and to cover them. But we have not yet attained these mechanical limits. Mr. Alvan Clark is ready to work glasses of 59 inches in diameter if M. Feil can supply him with them, which he certainly will not refuse to attempt. Our mechanicians, M. Gautier and the MM. Brunnet, know thoroughly well how to mount them. What, then, are the obstacles which stand in the way, and cause the major part of the great instruments to show barren results—the great reflectors, above all, to fall short of our expectations, and the experience of which compels the astronomer to confine himself to relatively small instruments, if he does not wish to wear himself out in efforts too often utterly barren? Two experiments may enable us to understand.

I have had a genuine reflector arranged by M. Molteni, with which we are going to observe together. A silvered mirror receives the rays emanating from a luminous object, gathers them into an image, and this image, magnified by a lens, is projected on to a screen. It is extremely sharp. I took an absolutely perfect mirror of L. Foucault's. Now with a spirit-lamp we very slightly warm a part of the back of the mirror. Immediately the image is confused. The details become invisible. We cease to warm it, and almost instantly the details reappear.

If you have an opportunity of viewing a bright star in a reflector with a glass mirror, you can easily repeat this experiment. While you are looking through the eye-piece, if another person places his hand on the back of the mirror, immediately you will see the image of the star take the strangest forms, and return to its pristine form when the hand is removed. The slightly-warmed superficial stratum of the mirror expands, and forms in effect a wedge which buries itself in the substance of the mirror, the reflecting surface changing its form the more it is stretched. Hence the distortion of the image.

A mirror, then, only remains perfect under the conditions of being everywhere of identical temperature. It is the same with an object-glass, save that the effect of a change of temperature is less energetic. But during observing nights it most frequently happens that the temperature is constantly changing; the different parts of the mirror of a telescope are very unequally exposed to the contact of the air and to radiation towards surrounding objects. It is rarely, indeed, that such a mirror rigorously preserves its parabolic form, especially if it is a glass one, and very thick. During the years that I have observed with the great Parisian telescope, I have found but one solitary night when the mirror was perfect; the sky had remained for two days half clouded up, and the night temperature was the same as that of the day.

A metallic mirror is a better conductor of heat, and will be less exposed to this disastrous effect; but it is very much heavier, although generally thinner. Its own weight then becomes a cause of distortion, although they have tried to overcome this by very complicated and ingenious systems of springs and levers, on which it was caused to rest. It is admitted that mirrors are very unequally good according to the positions which they assume during observation. Object-glasses demand almost equally great precautions to prevent their distortion in their mountings. As long as they are of moderate dimensions, the rigidity and elasticity of glass suffices to preserve the form given to them by the optician; but it is a different thing when their diameter becomes considerable, their thickness not increasing in the same proportion. I remember when the magnificent 15 inch object-glass of the Bordeaux Observatory arrived from Munich, the screws which kept the pressure-springs of the cell in place were slightly deranged during the journey. The images which this objective gave were desperately bad, and it required long trials to restore them to their normal form. Suppose that discs of glass of 27·6 to 31·5 inches in diameter, whose thickness does not exceed 1·6 inch or 2 inches, and is often less, and you will understand the vexatious effects produced by such an objective on images on the least distortion of its cell.

I cannot avoid telling you here one of my chief fears on the subject of the future of large object-glasses. I have told you that the solitary one of these great glasses which has been really used, that at Washington, recognised originally as very fine by Mr. Newcomb, seems now to inspire misgivings in Mr. Asaph Hall. Is it not possible that its shape has changed? M. Peligot, the illustrious chemist, of La Monnaie, had a perfectly straight glass rule in his office. It hung for a year out of the perpendicular by one of its extremities, at the end of which time it had taken a pronounced permanent curve. I have seen an excellent mirror which had been shut up for some months in a wooden box, come out completely warped and distorted, because the box had been under the influence of moisture. H. Sainte-Claire Deville said that glass is extremely like sealing-wax; it is as easily distorted if not properly supported. What will become, after some years, of such great thin discs as form the great object-glasses, and which are held at three points of their circumference only. I confess that it would be rash to affirm that they will preserve, for any length of time, the form which the optician has had so much difficulty in giving them.

Let us return to our telescope of projection, and now pass the flame of the spirit-lamp beneath the pencil of rays reflected from the mirror. Immediately you see the image is agitated, deformed, and becomes unobservable. Well, when a reflector or refractor is directed towards a star, the rays which the instrument receives have traversed all the thickness of the atmosphere before reaching it. If the objective or the mirror is small, the cylinder of air which these rays traverse may well be of equal density at every point of a transverse section. On the contrary, changes of density will be much more probable and more mischievous as the section of this cylinder becomes larger. A small instrument gives quiet images of a star, when at the same time a large reflector will only show images of that star plagued by continual distortion; and this annoying effect is more sensible in reflectors than in refractors, since with equal apertures a refractor gives in general better images than a reflector, for three reasons: because the objective is less distorted by changes of temperature than the mirror; because the effect of these distortions is much less; and finally, because the aerial agitations are less felt. In passing, you see, how unjust it would be to condemn a large mirror solely because at a time when it is giving indifferent images a smaller instrument is exhibiting fine ones. But, above all, you will see the explanation of the fact that we have deduced from experiment, that the practical power of an astronomical instrument increases with the diameter of the object-glass, or mirror, up to a certain point, and beyond that, on the contrary, it apparently decreases. The ideal power increases entirely in proportion to the augmentation of the diameter, but at the same time the causes of perturbation increase also, and at a more rapid rate, so that they finish by counteracting and completely annulling the useful effect of an increase in diameter.

I think that, stated now in these terms, the question of the superiority of large instruments over small ones, or small ones over large ones, becomes sufficiently clear, and should receive a rational and logical solution. For the ordinary purposes of observation we must not exceed a certain size of instruments. A refractor of 15 inches, or reflector of 16 inches, will be the ones by whose aid the astronomer will easily see all that has hitherto been discovered in the heavens, and be able to profit by all nights clear enough for observation. I do not mean to say that smaller telescopes will be without use for the observer who wishes to use them intelligently; he may even make some interesting discovery by their aid; but experience has taught us that everything new observed in small instruments requires to be verified by the aid of larger ones before being admitted as a scientific fact.

But it does not satisfy our curiosity to observe what is already known. Of course the really useful work of an observatory consists in the determination of the positions of known stars, and in the investigation of the laws of their movements. For this apparently thankless task ordinary instruments suffice; but it is also a noble and healthy ambition which prompts the human mind to extend the limits of the universe for the better understanding and admiration of the omnipotence of the Creator. But for this seeking of the unknown, telescopes of even 15 inches in aperture are insufficient. We are no longer in that fruitful epoch when Galileo, Huyghens, and the Cassinis had only to direct their imperfect telescopes to the sky to discover wonders of whose existence even the human eye had no suspicion. For finding anything fresh in some unknown corner of the universe we now require all the power of the largest instruments we can construct.

The astonishing progress of celestial photography has induced the belief that it has rendered useless the employment of these great instruments, and make up by the length of exposure for the faintness of the stellar light. But we need not consider long the relative parts played by photography, and by direct observation, to understand that the one will never replace the other. We must have these large refractors; but we have become acquainted with the difficulties created by their use. We must, above all, place them under such conditions that the obstacles proceeding from their mode of mounting, and their atmospheric surroundings, shall hinder, as little as possible, the free exercise of their optical power. Reflectors seem just now out of favour. They can, at present, only be used under restrictions. It is, then, useless to construct new ones until we can remove the evil influences of variations of temperature and of atmospheric undulations. Mr. Common is so engaged in England; his success in practical astronomy allows us to hope that he may succeed. We must then make a few very large refractors, and mounting them mechanically perfectly, use them under the purest sky, perhaps at very great heights. The erection of the great equatoreal at Nice seems to fulfil these desiderata, and promises a harvest worthy of the efforts of its generous donor. And even this is not all;

perhaps the most important point is that the observer entrusted with one of these great instruments should be animated with unshaken zeal, and should possess a strong dose of patience. Possibly years may pass before his researches are crowned with success; it was only by indefatigable perseverance that Mr. Asaph Hall succeeded in finding the satellites of Mars; but at that instant the enormous cost of his instrument, his long vigils, and all his trouble—all were amply repayed!

SELENOGRAPHICAL NOTES, SEPTEMBER, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Reiner and the Neighbourhood. That portion of the Oceanus Procellarum extending from N. lat. 2° to N. lat 15°, and from E. long. 45° to 62°, includes two considerable ring-plains—Reiner and Marius—very similar in type to many others in the same quadrant, associated with features which relieve the otherwise monotonous character of the region, and render it worthy of more than a hasty survey.

Reiner, the more southerly of the ring-plains, about 21 miles in diameter, presents nothing specially worthy of notice beyond a well-marked terrace on the inner slope of its eastern wall, and a small but very distinct central hill, which, on September 24, 1885, at 9h., when the moon was just past full, was seen to stand on a narrow light streak running from north to south, and extending nearly from wall to wall. Schmidt shows no indication of this object, which is probably a low ridge or swelling visible only under a very high light. A short distance E.N.E. of Reiner there is a remarkable white quadrangular marking, which from the time of Riccioli has been noted and drawn by various selenographers. It is marked Γ (Gamma) in Neison's Map XI., and is described as a plateau scarcely 200 feet high. Schmidt, alluding to its appearance on September 14, 1848 in the 8-foot heliometer at Bonn, regards it as a large wall-surrounded area, not however visible as such when near the terminator. Subsequently, on September 17, he remarked a smaller ring-shaped object connected with it on the western side, of which I can find no notice elsewhere, though a light circular spot, forming the western termination of a white streak which traverses the quadrangular area, probably answers to it. There can, I think, be little doubt that this large white spot, which at full moon is the most noteworthy feature in the neighbourhood, was named by Riccioli Galilæus, and not the small object now known as Galilai on the maps, which lies some miles further towards the north-east. A glimpse at Mayer's and other old charts, renders this almost indisputable. Schroeter (*Sel. Top. Frag.* T. XXXI.) draws it resembling a boomerang in shape and of uniform tone throughout, though the interior is clearly somewhat darker than the borders. Though this area is said to represent the site of a plateau 200 feet above the general level of the Mare, I have failed to distinguish any indications of the fact even when it has been most favourably placed for displaying its contour by shadow. My observations were, however, confined to morning illumination. It would be well to observe it towards sunset, though Schroeter's T. XXXI., which was drawn under these conditions, shows no trace of a shadow border. In this drawing Reiner is represented as a deep pit full of shade, and with no trace of an outer slope. Observations made at sunrise, however, show that the border rises to some height above the plain, albeit it is nowhere lofty. Between 9h. and 10h. on August 11 the phase was almost identical with that shown in Schroeter's T. LX., fig. 1, and one was able to compare the drawing with the moon, and duly appreciate its rugged accuracy. The western glacis of Reiner stood out beyond the terminator in the form of an ill-defined white and slightly curved streak. North of it, and forming part of the terminator, was noted what seemed to be a segment of a ring, which, if complete, would rival Reiner in dimensions. It is prominently shown by Schmidt, *Sect.* XIX., and by Mädler, and from its appearance seems to be of a distinct character from the ridges which are associated with it. Reiner A and C are two noteworthy craters lying S.W. of the ring-plain. The former at this stage throws a prominent shadow, and is clearly the larger and loftier object of the two, though C casts a conspicuous cone of shade. The more conspicuous ridges hereabouts are accurately depicted by Schroeter, whose work, as far as it goes, could hardly be improved upon as regards the qualities which constitute a drawing of permanent value. Reiner B, and a cluster of smaller craters, all with very brilliant rims, lie a few miles W. of A and C. Even Schmidt does not show all that a moderate power brings to light. It would be good practice for a beginner to try his hand at mapping these objects on a fairly large scale. One of the most far-reaching and attenuated of the bright rays from Kepler strikes across this region in a bold curve, convex towards the south, and terminates at Reiner C—at least so I have remarked it on several occasions, though Mädler and Schmidt draw it as terminating considerably south of this point. Another Kepler ray, broad, conspicuous, and figuring in the rudest maps, abuts on the W. wall of Reiner, crossing on its way the anomalous incomplete ring noted above. A very wide and lofty ridge, originating near the south-east wall of Marius, follows a somewhat indirect course to a point a little west of Reiner A. A pair of minor ridges, originating near the incomplete ring, coalesce and join the first not far from its southern termination, the point of junction being marked by a conspicuous crater which is omitted by Neison and doubtfully indicated by Mädler. There are two small craters on the plain slightly west of Reiner, about five or six miles from the border; one of them, the more southerly, is only shown by Schmidt, the other is not recorded in any map. A ridge of very moderate height extends from the southern wall of Reiner towards the south, traversing in about N. lat. 5°, a region dotted with craterlets, which under a high sun appear as white spots. There is a row of three craters of a larger type running in a meridional direction N. of Reiner: from their appearance at sunrise they seem to be lofty cones.

Kempston, Beds.: August 16, 1886.

SIDEREAL NOTES FOR SEPTEMBER, 1886.

BY J. E. GORE, F.R.A.S.

The following variables may be well observed in September:—

1. T. Herculis. R.A. 18h. 4m. 47s., N. 31° 0′ (1886·0). At maximum the magnitude varies from 6·9 to 8·3, and at minimum from 11·4 to 12·1. The mean period is 165·1 days, and a maximum will be due on September 5. The star may be easily found at maximum with a small telescope, as it lies between 99 and 104 Herculis, nearer to the latter star. It may be distinguished by its very reddish colour.

2. R. Delphini. R.A. 20h. 9m. 25s., N. 8° 45′ (1886·0). A variable found by Hencke, August 16, 1851, and mistaken for a new planet. In 1859 Schönfeld discovered it to be variable. The magnitude at maximum varies from 7·6 to 8·5, and at minimum the star descends to 12·8 mag. The mean period is 284 days, and a maximum is due this year during the last week in August, or first week in September. Observations of this star are desirable, as it does not seem to have been much observed of late years. It lies about midway between β Aquilæ and ε Delphini, and about 35′ preceding a 6½ mag. star Lalande 38936.

3. U. Cygni. R.A. 20h. 16m. 4s., N. 47° 32′ (1886·0). A variable, discovered by Knott in 1871. It varies from 7·7 at maximum to below 11 mag. at minimum. The period is very long, about 461 days. A maximum will be due this year about September 7, and observations are very desirable, to enable the period to be determined with greater certainty. The star is well worth looking at, as it is of a remarkable ruby colour, and was described by the late Rev. T. W. Webb as showing "one of the loveliest hues in the sky." It may be easily found, as it lies about 40′ *following* the 4½ mag. star 32 Cygni. There is a blue star near it.

Attention may again be directed to the new variables mentioned in the notes for June, as observations are very desirable.

Minima of Algol may be observed on September 11 at about 10h. 58m. p.m., and on September 14 about 7·40 p.m.

The following "suspected" variables are especially worthy of observation in September:—

1. Lalande 36099 Aquilæ. R.A. 19h. 7m. 50s., N. 5° 19′ (1880·0). 6½ Lalande; 7½ Bessel; 6 Argelander and Heis; and 6·4 to 6·7 at Cordoba. My observations, 1884—1885, vary from 6·6 to 7·0. The star seems certainly variable to a small extent.

2. Lalande 36606 Sagittarii. R.A. 19h. 19m. 21s., S. 21° 29′ (1880·0). Rated 8 mag. by Lalande, Piazzi, and Harding; 6·5 by Argelander, October 17, 1852. In the summer of 1851 it was seen about 9 mag. It is not in the *Uranometria Argentina.*

3. Lalande 36863 Aquilæ. R.A. 19h. 24m. 9s., N. 2° 39′ (1880). 7 mag. Lalande, 6–7 Heis. The Cordoba estimates vary from 6·3 to 7·2 mag. My observations, 1884-85, vary from 6·8 to 7·1, but are not sufficiently numerous to decide the question of variation. I have some suspicion that this may prove a variable of short period.

4. Birmingham 558 Delphini. R.A. 20h. 19m. 58s., N. 9° 40′ (1880). Rated 7, 6, and 6½ by Lalande. Birmingham's estimates (1872—1876) vary from 6 mag. to invisibility. The Cordoba estimates are 6·4, 6·6, and 6·7. Lynn states that he observed this star on October 16 and 29, 1872, and that on the latter date it was nearly 2 magnitudes fainter than on the 16th. Schönfeld includes it in his provisional list. My observations are as follows:—October 7, 1879, about 6·3 mag.; November 10, 1879, 6·3; September 18, 1884, 6·4; September 10, 1885, 6·5; November 14, 1885, 6·5.

5. Anon. Capricorni. R.A. 20h. 23m. 51s., S. 12° 38′ (1880). Not in Lalande or Harding. Variously estimated at Cordoba

from 6·8 to 8½ mag. I have observed the star as follows:—September 13, 1884, much less than Lalande 39361 (7·6 mag. Gould); August 13, 1885, small with binocular in a clear sky; September 7, 1885, small; September 9, 1885, small. In all these observations the star was certainly *below* 8 mag. in the scale of the *Uranometria Argentina.*

6. Pegasi. R.A. 0h. 16m. 4s., N. 6° 21′ (1885·0). Announced by M. Borrelly in 1885 as variable from 7 mag. to 9 mag. On December 22, 1885, I estimated it 8 mag. It may be easily found with an opera-glass, as it lies south of 41 and 45 Pegasi, forming roughly an equilateral triangle with these stars.

7. 100 Piscium. R.A. 1h. 28m. 39s., N. 11° 58′ (1885.0). Announced by M. Borrelly in 1885 as variable from 7 mag. to 9 mag. It was rated 8, 8½, 9, and 10 mag. by Lalande. My observations (September—December, 1885) confirm the variability, and show a maximum (7·2 mag.) about November 30, 1885. This star deserves careful attention. It lies closely *n.p.* the 5½ mag. star π Piscium.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.
To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE STARS B.A.C. 4632, 4628, *AND* 4627—σ^2 *URSÆ —STELLAR PHOTOGRAPHY—THE RAY-SYSTEM OF PROCLUS—PICARD* μ.

Sir,—The star B.A.C. 4632 is included in Mr. Gore's "Catalogue of Suspected Variable Stars" (No. 418) on the authority of Mr. Franks. He found it "splendid orange, 5 mag.," on April 7, 1878. In "Celestial Objects" B.A.C. 4628 is included, upon the authority of the same observer, as an orange-red star. On the 30th of June last I examined these stars, as well as B.A.C. 4627, which lies just *n.p.* 4628. I found B.A.C. 4632 small 5 mag. and fine orange. B.A.C. 4628 was "small 6 mag., and fine ruddy orange." The star B.A.C. 4627 was found to be 0·4 less than 4628, and bluish-white. The three form, in a low power, a beautiful group; a miniature of the well-known configuration in the head of Aries. Forming a right angled triangle with 4628 and 4627, *north*, lies a small, wide, 9 mag. pair, in which the southern star is the brighter.

Both in "Celestial Objects" and Mr. Gore's Catalogue, σ^2 Ursæ is inserted as "probably" variable, but no particulars or references are given in either work. I have obtained a few observations of this star, which I give below :—

July 20, 1885.	About 0·5 brighter than σ^1.
Mar. 27, 1886.	A half-magnitude above σ^1. Pale yellow.
May 14, 1886.	5 mag. estimated. ρ Ursæ also estimated at 5 mag., but not quite as bright as σ^2. σ^1 estimated at $5\frac{1}{3}$ mag. ρ and σ^1 were orange, and σ^2 pale yellow.
Aug. 1, 1886.	σ^2 0·4 mag. above σ^1, and about equal to ρ. σ^1, pale orange; σ^2, yellow; ρ, rich red gold.

The Photographic Exhibition here has given me an opportunity of examining some specimens of MM. Henry's stellar photographs, and of the work of other eminent astronomical photographers. The work is exceedingly beautiful, and almost up to what I expected to find it, but the respect in which it falls short is a point of interest to which I would like to be permitted to draw attention. Having seen so much as to the marvellous *circularity* of the discs in these stellar photographs, it surprised me not a little to find that this circularity was only preserved towards the centre, and that towards the corners the stars were mostly drawn out into sharp-pointed egg-shaped figures, with their axes directed towards the centre. I cannot say how many hundreds of the star-discs near the corners of the large plates of portions of Cygnus showed this curious malformation. It is clear that if this is a condition generally prevalent in these photographs, that some at least among the extravagant hopes founded upon them can never be realized.

I have to thank Mr. Gore for the information he gives *re* B 118; but in inquiring about the red stars I named I was not thinking so much about magnitude, as colour, observations.

It is often said that the lunar ray-systems are difficult to see except under high illumination, and in especial among the greater ones, that of Proclus is mentioned in that connection. It may be of interest to note that in regard to this system I have never found this to be the case, but have been able to trace some, at least, of the rays from shortly after sunrise to shortly before sunset. Thus, on the 16th instant, although conditions were very poor indeed, I saw the ray which passes between

Peirce and Peirce A. (Dennet's VIII.), and the ray passing between Picard and Peirce (Dennet's VII.), quite plainly, and likewise something of Dennet's VIII. The northern edge of the first-named seemed just to graze the south edge of Peirce A.; and the second seemed to reach Schroeter's forked ridge. The terminator lay within the western edge of the Mare Crisium, just upon the long low ridge which extends along the western floor. The night previous, when conditions were even worse, others of the rays were seen, though on neither night was X seen. Picard μ was very small and faint on the 16th.

I am, &c.,
S. MAITLAND BAIRD GEMMILL.
Glasgow: August 18.

REVIEWS.

Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditure, and Condition of the Institution for the year 1884. Washington: Government Printing Office. 1885. Pp. xxxvii., 904.

It goes without saying that an Institution so richly endowed and well managed as the Smithsonian continues in a most flourishing condition. The report of the Executive Committee shows that, whereas Smithson's original bequest in 1846 amounted to but little over 515,000 dollars, other bequests and savings have raised the total permanent Smithson Fund to 703,000 dollars, bearing interest at 6 per cent. per annum. It is satisfactory to know that though the materials used in the original structure were for the most part bad, such alterations have been made that the architects are able now to report that the principal parts of the Smithsonian building are, from foundation to roof, beyond the reach of any serious fire. To insure total immunity from fire, the reconstruction of the west wing is needed. This part of the report is accompanied by several plans and views of the building. The report of the Secretary, Professor Baird, gives details, in its 98 pages, of the general work of the Institution—which now includes the National Museum—during 1884. A lengthy appendix gives an account of the operation of exchanges, which appears to progress very satisfactorily, a large number of companies granting the privilege of free freight on parcels and boxes bearing the stamp of the Smithsonian Institution. But to an outsider the most interesting portion of the volume is the general appendix, which contains a record of scientific progress during the year 1884 in astronomy, seismology, geography, meteorology, physics, chemistry, mineralogy, palæontology, zoology, and anthropology, besides various illustrated papers relating to the last-named subject. The record of the progress of astronomy is from the pen of Professor E. S. Holden, the Director of the Washburn Observatory, and contains abstracts of the most important papers of the year that have appeared in various scientific journals. In connection with the very difficult work of determining the parallax of fixed stars, it may be interesting to compare the determination of that of 61 Cygni recently made with some of the older records :—

		″
1812.	Arago and Mathieu, by absolute altitudes ...	0·55
1840.	Bessel, by the heliometer	0·348
1846.	Peters, absolute measures	0·349
1853.	Pogson, by the heliometer	0·384
1863.	Auwers, micrometrically	0·564
1884.	Ball, from differences of declination	0·4676±0·0321
1884.	Socoloff using Schweizer's observations of 1863-6	
	From measures of distance	0·4598±0·0550
	„ „ position	0·4222±0·0342
	„ combination of measures	0·4330±0·0291
1884.	Gill	0·50

This part of the volume concludes with a most useful list of works on astronomy published in 1884, with their prices. The records of the other sciences are beyond our province.

POSITIONS OF THE NEW STAR NEAR χ^1 *ORIONIS.*

The following determinations of position were made by Professor W. A. Rogers, with the meridian circle of the Harvard College Observatory :—

Date.			*α* 1885·0			*δ* 1885·0		
			h.	m.	s.	°	′	″
1885	Dec.	16	5	48	59·58	+20	9	13·9
		20			59·61			13·1
		29			59·52			13·6
1886	Jan.	14			59·41			14·9
		17			59·55			11·4
		23			59·50			12·5
		26			59·49			12·1

Astron. Nach., No. 2738.

DUN ECHT CIRCULAR, No. 124.

Professor Krueger telegraphs from Kiel that Winnecke's periodic comet has been found at Capetown. It is described as circular, less than 1′ in diameter, as bright as a star of the tenth magnitude, and as having some central condensation, but no tail.

Its observed place was :—

Greenwich M. T.	R. A.	Decl.
Aug. 20, 5h. 47m. 54s.	13h. 10m. 21s·5.	S. 1° 8′ 17″.

The daily motion according to Lamp's ephemerides is about *plus* 3m·3 and 32′ *south.*

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt : 1886, August 23.

Books received :—Symon's British Rainfall. London : E. Stanford, Charing Cross.—Revista do Observatorio. Rio de Janeiro : Lombaerts & Co. 1886.—Sirius. No. 8. 1886.—L'Astronomie. Par Camille Flammarion. No. 8.—Bulletin Astronomique. Par M. F. Tisserand. Paris : Gauthier-Villars. 1886.—Sidereal Messenger.—Smithsonian Report. Washington, 1887.—Publications of the Leander McCormick Observatory of the University of Virginia. Vol. I., Part 2. 1886.

OBSERVATION OF THE OCCULTATION OF JUPITER AND HIS SATELLITES, APRIL 16, 1886.

The following are the sidereal times of observation of this phenomenon, as seen by Mr. Tebbutt at the Windsor Observatory, New South Wales, with the 4½-inch equatorial :—

Phase.	*Sid. Times.* h.	m.	s.
Total disappearance of Satellite I.	10	52	56·3
" " " II.	10	55	46·0
First contact of planet	10	59	25·8
Total disappearance of planet	11	1	12·3
Reappearance of Satellite II.	12	5	53·6
" " I.	12	7	56·1
" Planet's W. limb	12	11	55·1
Last contact of planet	12	13	38·1
Reappearance of Satellite III.	12	23	2·6

The second satellite occupied about $1^{s}\cdot5$ in disappearing, but the first disappeared more quickly. The disappearances of the third and fourth could not be seen owing to cloud. The reappearances were well observed, except that of the second, which was noted several seconds late. The objects were steady and well-defined throughout.—*Astron. Nach.*, No. 2736.

COMET BROOKS *a*, 1886.

The following elements have been computed by M. Lebeuf from observations on April 30, May 4, 8 :—

$$\begin{array}{rcl} T & = & \text{1886, June } 7\cdot5158 \text{ M.T. Paris.} \\ \left.\begin{array}{r} \Omega \\ \omega \\ i \end{array}\right. & \begin{array}{c} = \\ = \\ = \end{array} & \left.\begin{array}{rrr} 193^{\circ} & 1' & 29''\cdot5 \\ 33 & 42 & 7\ \cdot1 \\ 87 & 47 & 34\ \cdot7 \end{array}\right\} \text{Mean Equinox } 1886\cdot0. \\ \log q & = & 9\cdot439104 \end{array}$$

Bulletin Astronomique, May, 1886.

COMET BROOKS *b*, 1886.

Professor Wendell gives the following elements of the orbit of Comet, Brooks *b*, 1886, from observations on May 1, 3, and 6 :—

$$\begin{array}{rcl} T & = & \text{1886, May } 5\cdot02796 \text{ G.M.T.} \\ \begin{array}{r} \pi - \Omega \\ \Omega \\ i \end{array} & \begin{array}{c} = \\ = \\ = \end{array} & \left.\begin{array}{rrr} 39^{\circ} & 46' & 12'' \\ 288 & 34 & 1 \\ 101 & 1 & 39 \end{array}\right\} \text{Mean Equinox } 1886\cdot0 \\ \log q & = & 9\cdot92562 \end{array}$$

Astron. Nach., No. 2736.

COMET BROOKS *c*, 1886.

From observations on May 25, 28, 31, Mr. H. V. Egbert has computed the following elements :—

$$\begin{array}{rcl} T & = & \text{1886, May } 28\cdot76 \text{ G. M. T.} \\ \begin{array}{r} \pi - \Omega \\ \Omega \\ i \end{array} & \begin{array}{c} = \\ = \\ = \end{array} & \left.\begin{array}{rr} 170^{\circ} & 13' \\ 46 & 03 \\ 16 & 59 \end{array}\right\} \text{Mean Equinox } 1886\cdot0 \\ q & = & 1\cdot5122 \end{array}$$

Science Circular, No. 70.

BROOKS' COMETS, a, b, c, 1886.

The actual dates of the discovery of these comets are (in civil reckoning):—Comet *a* on the evening of April 27; Comet *b* on the morning of May 1; Comet *c* on the evening of May 22, though the last was not verified till the following evening by the detection of motion.

SATURN'S RING (*according to Besset*).

Sept. 18. Major axis of ring = 39″·83
Minor „ = 15″·64
Inclination of earth to plane of ring = 23° 7′·3 S.
Mean obliquity of the ecliptic, Sept. 17 = 23° 27′ 14″·35
Apparent „ „ „ „ = 23 27 6 ·38
Semidiameter of the sun = 15 57 ·6
Parallax of the sun = 8 ·81

Sirius, July, 1886.

THE PLANETS FOR SEPTEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	9 36 33	N.14 13	7″·2	22 50·5
	9th	10 20 54	N.11 51	5″·8	23 3·3
	17th	11 15 43	N. 6 44	5″·2	23 26·5
	25th	12 9 31	N. 0 34	4″·8	23 48·7
Venus ...	1st	9 17 2	N.16 34½	10″·9	22 31·1
	9th	9 55 54	N.13 41	10″·7	22 38·3
	17th	10 33 54	N.10 22	10″·4	22 44·8
	25th	11 11 11	N. 6 44½	10″·3	22 50·5
Saturn ...	11th	7 26 48	N.21 37	15″·6	20 1·8
	17th	7 28 54	N.21 32	15″·8	19 40·3
	25th	7 31 23	N.21 28	16″·0	19 11·3

Mercury rises an hour and three-quarters before the sun, at the beginning of the month, the interval decreasing.

Venus rises about three hours after midnight on the 1st, the interval increasing.

Saturn rises half an hour after midnight at the beginning of the month, and afterwards earlier each night.

Dr. Backlund announces (*Astronomische Nachrichten*, No. 2727) that he has resumed his researches on the theory of Encke's Comet. The first part, relating to its return in 1885, has just appeared; the second, treating of the comet's motion since 1865, will soon be presented to the Academy of Sciences at St. Petersburg; while the third, which is in preparation, will comprise the period 1819—1868.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Wed	1		Sidereal Time at Mean Noon 10h. 42m. 17·46s.			12 14·8
Thur	2	2	Conjunction of Moon and Mars 5° 26′ S. Sun's Meridian Passage 0m. 26·81s. before Mean Noon			12 10·9
Fri	3	9 19	Occultation of γ Libræ (4½)			12 6·9
Sat	4	19 55	☽ Moon's First Quarter			12 3·0
Sun	5					11 59·1
Mon	6			Jupiter's		11 55·2
Tues	7	9 43 10 33	Occultation of B.A.C. 6536 (6½) Reappearance of ditto			11 51·2
Wed	8			Satellites		11 47·3
Thur	9					11 43·4
Fri	10	8 11 9 12	Occultation of B.A.C. 7487 (6½) Reappearance of ditto	invisible.		11 39·4
Sat	11					11 35·5
Sun	12	22 50 13 50	○ Full Moon Near approach of φ Aquarii (4½)			11 31·6
Mon	13	8 33	Near approach of 24 Piscium (6½)			11 27·6
Tues	14					11 23·7
Wed	15	19 12	Near approach of ν Piscium (4½) Illuminated portion of disc of Venus=0·940 Illuminated portion of disc of Mars=0·919			11 19·7
Thur	16		Sidereal Time at Mean Noon 11h. 41m. 25·74s.			11 15·8

DATE.		Principal Occurrences.		Jupiter's Satellites		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	17		Sun's Meridian Passage 5m. 34·94s. before Mean Noon			α Pegasi — 11 11·9
Sat	18					11 7·9
Sun	19					11 4·0
Mon	20	17 55 11 34 12 30	☾ Moon's Last Quarter Occultation of 130 Tauri (6) Reappearance of ditto Saturn's Ring: Major axis=39″·96 Minor axis=15″·66	Jupiter's		11 0·1
Tues	21	11 26	Occultation reappearance of 26 Geminorum (5½)			10 56·2
Wed	22	10	Conjunction of Moon and Saturn 3° 29′ N.	Satellites		10 52·2
Thur	23					10 48·3
Fri	24	14 41	Occultation reappearance of B.A.C. 3345 (6)	invisible.		10 44·4
Sat	25					10 40·4
Sun	26	5	Conjunction of Moon and Venus 0° 34′ N.			10 36·5
Mon	27	9 18 8 15 23	● New Moon Conjunction of Moon and Mercury 1° 6′ S. Superior conjunction of Mercury and Sun Conjunction of Moon and Jupiter 2° 9′ S.			10 32·5
Tues	28					10 28·6
Wed	29	6	Conjunction of Uranus and Mercury 0° 34 N.			10 24·7
Thur	30	22 22	Conjunction of Uranus and Sun Conjunction of Moon and Mars 6° 11′ S.			10 20·8
OCT. Fri	1					10 16·8

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Sept. 1	+48	17M.	Sept. 11	−73	46M.	Sept. 21	−15	46E.
2	36	5	12	85	58	22	27	58
3	23	52	13	+81	50E.	23	40	9
4	11	40	14	69	38	24	52	22
5	− 0	32	15	57	25	25	64	33
6	12	45	16	45	14	26	76	45
7	24	57	17	33	1	27	88	56
8	37	10	18	20	50	28	+78	52M.
9	49	21	19	8	38	29	66	41
10	61	34	20	− 3	34	30	54	29.

Moon farthest from Earth, Sept. 11, 4h.
,, nearest to ,, ,, 26, 7h.

Erratum.—p. 216, l. 30, *for* ☊ *read* ☋.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To March, 1886.

Tupman, Col.

To June, 1886.

Campbell, Lieut.-Col.
Glaisher, J.
Jackson-Gwilt, Mrs. H.
Howlett, Rev. F.

To Dec., 1886.

Lancaster, W. L.
Parnell, J.
Thornthwaite, W. H.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

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The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 286. OCTOBER. 1886.

DIRECT PHOTOGRAPHY OF THE HEAVENS.

In a recent number of *L'Astronomie* is an article by MM. Paul and Prosper Henry, of the Paris Observatory, with two charts, one of the cluster in Perseus, taken October 10, 1884, after an exposure of 50 minutes, showing 550 stars from the seventh to the twelfth mags. The objective used was 6·3in. A new instrument has since been constructed, the objectives of which have been made by the MM. Henry. It is composed of two telescopes, side by side, enclosed in a metallic tube, and separated in the whole length by a thin partition. One of the objectives, 9·4in. aperture, and 11ft. 10in. focal length, is intended for eye-observations, and serves as pointer; the other of 13·4in. aperture and 11ft. 3in. focal length, is corrected for chemical rays, and is used for photography. The optical axes of the two telescopes being parallel, a star kept in the centre of the field of the former produces its impression on the centre of the sensitive plate of the photographic apparatus. The equatorial is mounted in the so-called *English* mode, that is, the centre of the tube is always on the polar axis of the instrument. This arrangement enables a star to be followed from its rising to its setting, without turning the instrument in the neighbourhood of the meridian. It has also the advantage of giving in all the regions of the heavens direct and reverse positions, which permits of the elimination of certain errors in centering. It is furnished, like ordinary equatorials, with an hour-circle, declination-circle, and clockwork, which can drive the apparatus for three hours without requiring to be wound up. There are independent very slow movements for adjustment, which allow of the axis of the telescope being kept in a perfectly fixed position, in spite of any slight irregularity in the clockwork, or in the orientation of the telescope. The photographic objective, the largest yet constructed, is formed of a single achromatic system, and although of very short focal length in proportion, it can cover with great clearness, and without employing any diaphragm, the very considerable field of 3° of diameter.

"Notwithstanding that this apparatus is not yet entirely in order, we have been able," say the MM. Henry, "to obtain, after an hour of exposure, the proof here reproduced. On a surface representing a surface of the heavens of about 5° square, we can count 2,790 stars, comprised between the fifth and the fourteenth mags. as clearly marked on the borders as on the centre of the proof. On the plate traces of stars of the fifteenth mag. can be distinguished, but too faint to bear the transfer to paper; they will certainly be obtained by longer exposure. Stars of the fourteenth mag. appear with a diameter of $\frac{1}{40}$th of a millimeter ($\frac{1}{1000}$th of an inch). Such a chart obtained in an hour would certainly have required several months of continuous labour by the ordinary methods."

MM. Henry give the duration of exposure for each stellar magnitude from 1 to 16. For example, a star of first mag. requires the 5,000th of a second; one of the sixth mag. half a second; the tenth, eleventh, and twelfth magnitudes, 50 seconds (the asteroids would be represented by these), whilst the fifteenth and sixteenth magnitudes, the limit of the grasp of the largest telescopes, require 1h. 23m. All these figures represent a minimum; to obtain good reproductions on paper, these times of exposure must be tripled. MM. Henry observe that this advance has sensibly increased the power of human vision, since it enables us to obtain the image of a star, or the trace of the path of a heavenly body that would be invisible with telescopes of the same aperture as those used for photography. The persistence of the impression renders the photographic plate more sensible than the retina.

L'Astronomie has the following remarks on this great achievement: "The first great problem that is likely to be solved in some years is the construction of an exact chart of the heavens, with the numbering, classification, and position of all the stars visible with large instruments, a problem which has long occupied the most celebrated astronomers, and which has hitherto been considered insolvable."

Herschel, after making his great telescope, devoted long labours to the solution of only a part of the question, the numbering of the stars. But he found by the processes of sounding or of gauging which he devised, that he would need

45 years of labour to complete the explorations of the heavens; nevertheless, after some years of "observation," he thought he could approximately determine the number of stars visible with his great telescope, that is as far as the fourteenth or fifteenth magnitudes, as being twenty and a half millions. By a remarkable coincidence we arrive at nearly the same amount with one proof, obtained by an hour's exposure. Since the surface of the sphere contains about 41,000 degrees square, if we assume that one proof represents a region of the heavens of mean density, the 2,790 stars contained in these five degrees square, give nearly twenty-two and a half millions of stars for the whole celestial vault.

As another study of great importance, rendered now possible by photography, we may mention the discovery of asteroids. The small stars inscribe themselves on the plate, we may say, like a mathematical point, but the planets are distinguished by a small perfectly clear line, indicating their proper motion in extent and direction during the time of the exposure. It is thus that we have already obtained, with the provisional apparatus of last year, the proper motion of Pallas amongst the fixed stars. In the same manner we shall be able to study the motion of satellites round their planets; on the proofs of Jupiter, taken every ten minutes, the course of these little bodies round the primary is distinctly seen.

The examination of double and multiple stars, when the groups are not too close, will be much facilitated; and photography may also be applied to the discovery of parallax; with an ordinary microscope, and a power of 15 to 20, points have been made on the plates to one or two tenths of a second nearly.

In the interesting question of the photometry of the stars, photography gives a different interpretation to that of the human eye, but one not less valuable. As is known, blue becomes white in photography, whilst yellow and red become black. White and bluish stars of the fifteenth and sixteenth magnitudes allow themselves to be photographed without difficulty: yellow, orange, or reddish stars are not photogenic. In the cluster of Perseus there is notably a star of the 8½ mag. which only imprints on the plate the mark of a star of the eleventh mag. The more brilliant red stars, Aldebaran for example, do not give discs large and dull, but minute points. Variability of colour will be revealed by photography. There are besides isochromatic plates (among others those of MM. Clayton and Atout-Tailfer) with which all stars of whatever colour can be photographed with their real intensity of light.

But, above all, the *proper motions* of stars will be manifested by periodical comparisons of the celestial aspects. Relative positions of great accuracy substituted for absolute positions will, in ten or twenty years, evidence displacements, which a hundred years of meridian observation could not have indicated. Every personal equation having been eliminated from the beginning, the results will be worthy of the greatest confidence. In conclusion, the ingenious labours of MM. Paul and Prosper Henry are of those which render illustrious at once an establishment and a country. They do very great honour to the observatory of Paris, and contribute in a special manner to its scientific prosperity and its glory.

SOME OF THE MORE REMARKABLY COLOURED SOUTHERN STARS.

For more than a century past the subject of the colours of the stars visible in European countries has engaged the attention of numerous observers, amongst whom may be found the names of many of the most noted astronomers, and this is in itself sufficient evidence of the importance attributed to this branch of observational astronomy. Red stars, or more properly those ranging in colour from yellow to red, have more especially been searched for and catalogued, and the colours of double stars have likewise received a large amount of attention, though stars of other colours and other kinds have been by no means altogether neglected. It is a matter for much regret, however, that our knowledge of the stellar colours is confined almost entirely to those stars which are visible in high northern latitudes. The neglect which one is unfortunately so accustomed to find in nearly all those branches of astronomical enquiry, which can only be properly undertaken by those dwelling in the southern hemisphere, is here experienced to an extent perhaps unusual, even in such branches themselves.

The principal sources of information as to the colours of southern stars appear to be the "Cape Observations" of the Second Herschel, and the "Uranometria Argentina" of Gould. The notes in the former work, however, are scanty, and refer chiefly to some instances of more striking colour. The results of the latter are very numerous indeed, but, from the manner in which the colours are registered, are usually incapable of giving much information as to the actual colours of the stars. The apparent partiality of some of the Cordoba observers towards red colours, moreover, also detracts somewhat from the value of the results, even as given.

During a recent sojourn in the southern hemisphere, I gave some attention, at odd moments, to this subject, and succeeded in securing observations of the colours of about 200 of those stars which are situate to the south of south declination 30°. From these, the stars given in the accompanying list have been selected, as being those in which the colours are more deeply marked; and indeed in this list will be found some of the most beautiful and strikingly coloured stars in the heavens. In this respect it should be observed, that a yellow star, in which the colour appears of an unusual degree of pureness and intensity, is as much worthy to be called one of the finest coloured stars in the heavens, as one in which the colour is more deeply tinged, or redder; and so with other tints. The determinations of the colours of the stars in this list, which are all visible to the naked eye, were made by means of an opera glass and a 2¾-in. refractor. The figures in the last column indicate the number of nights on which the colour of the star was determined. An asterisk has been affixed to those in which a red colour is sufficiently marked to entitle them, in the opinion of the observer, to be included in a red star catalogue, such as that of Birmingham. Finally, at the end of the list of coloured stars, are given a few notes on the colours, and sometimes on the magnitudes, of some of the stars contained in it. As might be expected in the case of stars remarkable for their colour, the magnitudes assigned by different observers are often very discordant.

Many of the discrepancies between the colours of stars, as estimated by different observers, are rightly attributed to well-known causes, such, for instance, as instrumental differences and personal equation, but it does not seem that sufficient consideration has been paid to apparent changes due to differences in the atmospheric conditions. Yet the apparent changes, which may reasonably be ascribed to such a cause, are both numerous and considerable in extent. In England, it will be found, that the almost universally maligned east wind is as much opposed to the proper estimation of star colours, as it is to most work of an astronomical nature, and so seem to be most winds of a very dry nature. On the other hand, warm, moist atmospheres have a tendency to render the stellar colours more pronounced, and especially have a marked effect upon white, yellow, or yellowish stars. In the very warm and very moist airs of some tropical climates, this atmospherical influence operates so strongly as to colour nearly all the stars, at least those visible to the unaided sight, more or less yellow or yellowish. The brilliant Sirius under such circumstances shines with a markedly yellow light. Definition, too, has a considerable influence, as it is much easier to determine the colours with satisfaction on a calm steady night than on one of an opposite character. It is clear, however, that there must be a limit to these apparent changes; and the subject of the apparent changes of colour in stellar objects, due to differences in the atmospherical conditions, is one well worthy of careful study.

List of Southern Coloured Stars.

No.	Name of Star.	Colour.	No. of Obs.
1	α Apodis ...	Deep yellow	1
2	β Arae... ...	Fine deep yellow	6
3	γ „	Bluish	3
4	ζ „	Deep yellow	4
5	η „ ... ...*	Deep orange yellow	4
6	ε Carinae ...	Deep yellow	6
7	ι „ ...	Deep yellow	3
8	r „ ...*	Reddish yellow...	2
9	q „ ...*	Reddish orange	2
10	α Centauri ...	Yellow	5
11	ψ „ ...	Bluish white	1
12	γ Crucis ...*	Orange red, very fine colour	9
13	β Doradus ...*	Reddish yellow	1
14	ε „ ...	Bluish white	1
15	β Gruis ...*	Orange	3
16	γ Hydri ...	Deep yellow	1
17	φ' Lupi ...*	Reddish yellow	2
18	ε Muscae ...	Deep yellow	1
19	α Pavonis ...	Bluish white	2
20	γ Pictoris ...*	Deep reddish yellow.. ...	1
21	π Puppis ...*	Deep orange red; very fine colour	28
22	c „ ...*	Reddish orange	6
23	L^1 „ ...	Bluish white	1
24	L^2 „ ...*	Orange red	17
25	κ Pyxidis ...*	Reddish yellow	1
26	γ Reticuli ...	Deep yellow	2
27	ζ^2 Scorpii ...*	Deep orange yellow	2
28	α Triang. Aust.	Deep yellow; fine pure colour...	5
29	ε „	Orange yellow	2
30	λ Velorum ...	Fine deep yellow	6
31	N „ ...	Fine deep yellow	3

Notes on some of the foregoing Stars.

5. η Arae is marked *c* in the "Uranometria Argentina," signifying that the star is strongly coloured with a hue in which red is not predominant. The magnitude is there given as 3·8, but the mean of two determinations in 1886 makes it 3·35: Lacaille made it 5m.

9. *q* Carinae =L.4249. Gould remarks that: "The markedly red star *q Carinae* has seemed to vary at least between the limits 3·3m. and 4·5m.; and certainly attains the brightness 3·3m. given in our catalogue. Lacaille noted its mag. as 4 in the zone, and as 5 in his catalogue; Behrman gives it as 4, and Ellery in 1869 made it 5·0." Herschel from his "Sequences," found the mag. 3·81. It was estimated 3·7m. by me on one night, and 3·75 on another.

10. α Centauri. The magnitude of this most magnificent double star, seen as one, is given as 0·7 in the U. A. Herschel from his "Sequences" made it 0·34: and from his photometric observations 0·59. From comparisons made on three nights, I found the magnitude to be —0·40 according to the photometric scale of the "Harvard Photometry."

12. γ Crucis. This superbly coloured star is No. 282, of Birmingham's Catalogue of Red Stars. Gould states the colour to be a clear orange yellow, and that its magnitude "has been variously estimated from 1·8 to 2·4, even by the same observer, and it is probable that the brightness of the star really changes by a considerable amount." He gives the magnitude as 2·0. Herschel from "Sequences" found 1·73, and from photometric observations 1·85. From careful comparisons on ten nights in 1886, the mean magnitude comes out 1·46; the individual determinations being very accordant, differing in the extreme by only 1/5th of a magnitude. Russell in 1871, called the large star only "pale yellow."

17. ϕ^1 Lupi. Marked *rr* (excessively red) in the U.A., and Gould remarks that it is "very red, which may account for the diversity of the magnitudes assigned to it by different astronomers." The mag. is there given as 3·6; Herschel from his "Sequences" found it 4·18; Lacaille estimated it of the 5 mag.; and on one night in 1886, I found it to be 3·45, and on another night 3·3.

18. ϵ Muscae. The Cordoba observations give the value 4·7m. to this star, the mean of many discordant observations from 4·3 to 5·3. Behrman estimated it 4½; Lacaille 6. My observations in 1886, range from 4·2 to 4·8, the mean being 4·57.

21. π Puppis. This beautifully coloured star is No. 171 of

Birmingham's Catalogue of Red Stars, having been noted very red by Schmidt. Herschel, with the 20-ft. reflector, only called it yellow; and in the U. A. it is only marked with a single *r*. Gore, in 1875, found the large star a beautiful orange, and in the end of 1885 and commencement of 1886, it was certainly one of the finest orange red stars in the heavens. It would seem from this that there is here a strong probability of variation in colour. It has also been suspected to vary in brightness, and my observations, made on about thirty nights in 1885–6, confirm this, the determinations varying from 2·3m. to 3·3m. This star may therefore now be placed upon the list of known variable stars.

22. *c* Puppis = B. 189. According to the Notes in Birmingham, the chief star was called orange by Herschel, and by Schmidt very red. The U. A. makes it deep orange.

23, 24. L^2 Puppis is a well-known variable star, the variability of which was discovered at Cordoba. It is of a fine orange red colour. Gould remarks that the star is red in all its stages, and remarkably so when faint. It is rather finely contrasted with the neighbouring star L^1 Puppis, the colour of which is bluish white.

25. κ Pyxidis = B. 217. A suspected variable, the Cordoba estimates ranging from 4·3 to 5·1. My observations confirm the suspicion of variability, the determinations extending from 4·5 to 5·3. Gould states the colour to be bright orange, and in Birmingham's Catalogue, it is said to have been called "roth," by Argelander; "orangegelb," by Secchi; and "yellow, flashing red," by Birmingham himself.

30. λ Velorum is an orange star, according to Mr. Gore, who remarks that it was called "blood red," by Mr. Pope, of Dunedin, New Zealand.

31. N. Velorum. According to Gould, this star has a peculiar yellow colour, contrasting markedly with that of the numerous red stars in its vicinity. It is suspected to be variable in brightness from about 3·4m. to 4·4m. In 1885–6, it was estimated 3·3 on one night and 3·2 on another.

In concluding, it is necessary to state that my estimations of magnitude were all made according to the scale of the "Harvard Photometry," which differs considerably from that of the "Uranometria Argentina," particularly for the higher and lower magnitudes.

A. STANLEY WILLIAMS.

SELENOGRAPHICAL NOTES, OCTOBER, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Briggs and its surroundings. At 9h. 30m. on the evening of September 11th the western wall of Otto Struve, then on the morning terminator, extended continuously as a bright strip from the east of Selencus to a point about equally distant from Briggs *b*, the position of Briggs A, which stands on the wall of the great plain, being indicated by a slight westerly bulge in its otherwise direct course. A prominent notch was noted in the wall slightly to the north of this, representing a hitherto unrecorded crater of considerable size which at full is marked by a brilliant spot. There is nothing specially remarkable about Briggs itself, except the long low ridge which traverses the interior in a meridional direction, swelling into a broad but not lofty eminence at the centre and gradually narrowing as it approaches the northern and southern limits of the floor. The wall of Briggs is not continuous, but shows a narrow opening on the extreme north. The western section is apparently of uniform height except at a point a little north of the centre, where its shadow indicates a sudden increase in its altitude. The eastern wall is far more irregular. At 9h. the shadow of its loftiest point extended midway between the centre of the ring-plain and the western wall of Otto Struve, which at that time was on the terminator. Briggs *b*, situated a short distance to the north-east, is a much larger and more important object than it appears to be on either Beer and Mädler's or Neison's map. Its brilliant eastern wall rises to a greater height above the plain than that of Briggs, and its interior is evidently deeper. West and south of Briggs there are many ill-defined nebulous craters, the largest being Briggs C. Neison (Map VIII.) shows three of these objects, but between the most westerly of the craters he represents and the ring-plain there are at least four others, one of which is double. At and about full moon Briggs and its companion ring-plain *b* figure as ill-defined oval light spots, except as regards their eastern walls, which are brilliant and distinct. Briggs A is conspicuous at this stage by reason of its abnormally dark floor. About midway between Briggs A and *b* there is a good sized craterlet, not shown in any of the charts.

The Ring-plain f south of Aristarchus, &c. When this object is on the morning terminator, as was the case at 8h. on September 9th, it appears to be intimately associated with ridges originating at the end of spurs running out from the south and south-western border of Aristarchus. The most westerly of these ridges is first traceable at the foot of a long spur pertaining to the south-western wall, from which point it trends southwards up to the western side of the ring-plain *f*, which seems to be merely a prolongation of it, and then proceeds onwards in the same direction. A second ridge, also originating at a spur from Aristarchus, running parallel to the last, forms the eastern wall of *f*, and curving round towards the west, joins the first, while a third, crossing the second and also joining the first, forms the northern border of the ring-plain. The interior of *f* is full of shadow at a time when these ridges cast only a very narrow fringe of shade, hence the formation cannot well be due to a mere accidental interlacing of these features, though the connection is remarkable. Schmidt shows two craters under the western wall of *f* on the floor, which I have not been able to identify as such, though a light spot was noted in this position on March 9th, 1884. Neison (Map VIII.) draws another ring-plain between *f* and the south-west wall of Aristarchus, and Mädler shows an object (also marked *f*) a short distance due east of the ring-plain *f*, which seems to answer to Neison's *e*, described by him as "a shallow ring-plain." I have not, as yet, succeeded in seeing any traces of these objects, which do not appear in Schmidt's map.

Schmidt represents the somewhat coarse valley on the western *glacis* of Aristarchus as consisting in part of rows of small contiguous craters. I had a fine view of it on September 9th, under a power of 384, when its structure reminded me of the west wall of Cleomedes on a smaller scale. It seems to be made up of a series of "pockets," or alternate widenings and contractions, rather than of a crater-row.

Lichtenberg. Neither Mädler nor Neison show the glittering companion crater on the east of Lichtenberg, which is a very noteworthy object when near the morning terminator.

Kempston, Beds. : Sept. 22, 1886.

SIDEREAL NOTES FOR OCTOBER, 1886.

By J. E. Gore, F.R.A.S.

The following variables may be well observed in October:—

1. R. Aquilæ. R.A. 19h. 0m. 53s., N. 8° 3'·5 (1886·0). At maximum the magnitude varies from 6·4 to 7·4, and at minimum the star descends to about 11 mag. The mean period is about 345 days, and a maximum will be due this year about the end of October. The light curve is very variable, and according to Schönfeld the increase of light from 9 mag. to

7 mag. is often extraordinarily quick. Its colour is very red, and the eminent Swedish astronomer Dunér finds a very beautiful spectrum of Secchi's third type, with very large and dark bands in all the colours. The variable may be readily found at maximum with a good opera-glass, as it lies a little *preceding* a line joining 18 and 19 Aquilæ, a little nearer the latter star. It is preceded by a line of small stars, of which the most western and brightest is rated 6·8 mag. in the *Uranometria Argentina*, and forms a good comparison star for the variable when near a maximum.

2. DM. + 8°, No. 3780. R.A. 18h. 32m. 51s., N. 8° 43'·5 (1885·0). A variable discovered by the Rev. T. E. Espin in April last. It is 8·6 mag. in the *Durchmusterung*, but observed by Espin as 7·7 on April 26. It gradually rose to a maximum of 6·8 on June 4. Since this date it has diminished in brightness, and on August 20 was estimated only 8·2 by Espin. On August 22 I estimated it 8·5 in a clear sky. The star is evidently a remarkable variable of long period, and is deserving of careful observation to enable its period to be determined.

3. Birmingham 535 ± Cygni = DM. + 47°, No. 3031. R.A. 20h. 5m. 58s., N. 47° 28'·9 (1885·0). Variation has also been detected in this star by Espin. It passed a minimum about the end of April of the present year. On May 14 it was 8·9, since which time it has increased, and towards the end of August had risen to 7·7. Birmingham found it 9 mag. in August, 1876. It *precedes* 32 Cygni by 5m. 57s., and is 9' north of it. This star also seems to be a long period variable, and observations of it are very desirable.

Minima of Algol may be observed on the following dates:—October 4, about 9h. 20m.; October 7, 6h. 9m.; October 24, 11h. 3m.; and October 27, at 7h. 52m. p.m.

The following "suspected" variables are especially deserving of observation in October:—

1. Birmingham 579 Cephei. R.A. 21h. 9m. 46s., N. 59° 37'·2 (1880·0). Rated 7 mag. by Piazzi, 8 mag. by Argelander, and 8·5 by Secchi. Birmingham's estimates, 1872—1875, vary from 6·5 to 7·5; 9 mag. Copeland, September 28, 1875; 8 mag. Burton, August 10, 1875, and 7·6, August 12, 20, and 31, 1876. It has a spectrum of Secchi's third type. According to Webb it lies closely *n.f.* the double star PXXI., 51 Cephei (which lies between η Cephei and the variable μ Cephei). On September 2 of the present year I estimated this star 7·3 mag.

2. Birmingham 596 Aquarii. R.A. 21h. 40m. 19s., S. 2° 46'·1 (1880·0). 6½ Lalande; 7·5 Bessel; 5–6 Secchi, July 15, 1868. Birmingham's estimates, 1873—1876, vary from 6

to 8 mag. It is 6·8 in the *Uranometria Argentina.* From observations, 1880—1883, Espin finds that the star is certainly variable, but that the variation does not exceed 1 magnitude with perhaps a period of several years. My own observations, 1877—1885, show fluctuations of light of about 1 magnitude. Dunér finds a very beautiful spectrum of the third type, with large and dark bands, especially in the blue.

3. Birmingham 600 Pegasi. R.A. 21h. 58m. 32s., N. 27° 46′·2 (1880·0). 7·7 Argelander; 8 mag. d'Arrest, July, 1874. Birmingham's estimates, 1873—1876, vary from 6·5 to 8. Dunér finds a beautiful spectrum of the third type, with dark and very large bands, especially in the blue.

4. Birmingham 610 Cephei. R.A. 22h. 18m. 37s., N. 55° 21′·4 (1880·0). 7·2 Argelander; 6·4 Schjellerup; 6·5 Birmingham, March 19, 1873. "On June 17, 1868, Secchi found this star very small, and suggested variability. On December 9 he searched twice, and found the largest star on the field of view no more than 8 mag." On February 17, 1884, I found it about 7½ mag. with binocular; April 27, 1886, I estimated it 7·3; September 2, 7·4 mag.

To observers possessing telescopes of over 7-inches aperture the following very interesting binary star may be recommended for measurement.

τ Cygni. R.A. 21h. 10m. 0s., N. 37° 22′ (1880·0), 3·7, 7·8 : 116°·3 : 1″·10 (Tarrant 1885·90). A double discovered by Mr. Alvan G. Clark in October 1874. It is in rapid motion, having moved about 60° since the date of its discovery. I have recently computed the orbit of this binary, and find a period of 53·87 years, with periastron passage 1863·99. According to this orbit the distance between the components will increase slightly during the next few years, the position angle decreasing, and measures of it are very desirable to enable computers to calculate the orbit with greater accuracy. It may, perhaps, be best measured in strong twilight.

REVIEWS.

British Rainfall, 1885. *On the Distribution of Rain over the British Isles during the year* 1885, *as observed at more than* 2,000 *Stations in Great Britain and Ireland, with Articles upon various branches of Rainfall Work.* Compiled by G. J. Symons, F.R.S., Secretary of the Royal Meteorological Society, &c. London: E. Stanford, 1886. Pp. 54 and 219.

Like many other commanders who have a large staff of volunteers working under them, Mr. Symons finds that an immense amount of labour is constantly devolving upon him. And this labour seems likely to increase. There are now so many meteorological observers, that he receives from about 1,500 stations records of the fall of rain on every day in the year. Suppose there are on the average 150 days with rain during the year, these returns alone contain at least 240,000 entries, and 18,000 columns of figures. In order to ensure as near an approach to accuracy as possible, the addition of every one of these columns is re-worked, for it is surprising how many errors occur even in simple addition sums. Well may Mr. Symons say that the work involved is what some would describe as appalling; certainly many a man, less energetic and less devoted to his subject than himself, would be daunted by it. "Sheer laziness" still causes many observers to neglect small quantities in the gauge, and describe days with rain as if none had fallen. Last year a legacy of £100 was left by Miss Nunes, with which 30 rain-gauge stations were equipped, nearly all of which continue in perfect order. Legacies can hardly be expected very often; but surely those who send Mr. Symons their rainfall records ought not to put it in his power to say that "not one observer in seven pays as much as £1 per annum, and that not more than three out of seven pay anything at all." Such a state of things is far from encouraging to the chief of a voluntary association, who has been at the work for upwards of 20 years, and *who does not draw money for the purpose from the public treasury*. It is to be hoped that in his next Report Mr. Symons may have a different tale to tell.

From measurements made at Camden Square twice daily for more than 20 years, Mr. Symons concludes that (1) in winter the nights are wetter than the days; (2) in spring and autumn there is not much difference; (3) in summer nearly half as much again falls by day as by night. The summer excess is probably due to a considerable extent to thunderstorms. A comparison of the Camden Square measurements with those made at Greenwich and Kew do not confirm all these results, but they concur in showing the great excess of rain by day in summer. There are so many difficulties attendant upon attempting to record at sea the amount of rainfall, that most persons regard it as hopeless. Surgeon-Major Black, however, who is an indefatigable worker, seized the opportunity afforded by a fortnight's yachting tour round the British coast of taking with him a rain-gauge and a vaporimeter, and the results obtained by him are recorded in the Report. The largest rainfall during the past year—197·92 in.—took place at the Stye, Cumberland, and the least—16·73 in.—at Leith.

It must be a source of considerable gratification to Mr. Symons to be able to record that, whereas in 1860 only 168 perfect rainfall records were published in "British Rainfall," the number in 1885 had mounted up to no less than 2,486.

The *Sidereal Messenger* for July contains an interesting letter written some years ago by Otto Struve to Mr. S. W. Burnham on double-star work. In this he gives as his opinion that in a double-star catalogue it would be desirable to accept for the principal stars 8·0 (according to Argelander) as the limit of magnitude; for the companion, without regard to magnitude, 32″ as the limit of distance, by which named distance the ratio of the physical to the optical double-stars, as experience teaches (*Pos. Med.* p. ccxl.) is, at least in the case of the brighter companions, very nearly 1 to 1. But beyond 32″ the ratio rapidly diminishes, while within that limit the number of optical doubles is proportional to the square of the distance, and it would soon increase to a figure well-nigh immeasurable. Beyond these limits it would be important to establish limits in respect to the magnitude of the companions. Exceptions should be made of those doubles for which, as, *e.g.*, Regulus, 40 Eridani, &c., the physical connection, in spite of the faintest of the companions, is already assured, or, at least, very probable. Struve points out that such a work, taking 8·0 as the limit of magnitude for the primary and reaching from the North Pole to 30° south declination, would require the setting the telescope upon, and careful examination of, about 80,000 objects. Readers of the *English Mechanic* will remember the wordy war that occurred between the late Rev. J. Pearson and Mr. C. L. Woodside respecting the computation of occultations by a method founded on a plan devised by the former gentleman. Mr. Woodside here gives another description of his method, for which he claims speed and a fair amount of accuracy. It is evident that the only way to decide the point is to work out the details of several occultations by this and other methods, and institute a careful comparison between them as to the degree of accuracy obtained and the length of time required.

Publications of the Leander McCormick Observatory. Vol. I., part 2. Tail of Comet 1882, II. University of Virginia, 1886. pp. 27 and plates 1–6.

This brochure contains notes of observations by Messrs. J. Jones and F. P. Leavenworth with the naked eye and field-glass of Comet 1882, II., and is accompanied by lithographic plates showing 42 representations of the Comet. The observations began on October 3, 1882, and the last was made January 14, 1883; they appear to have been made with great care and minuteness, though the length and breadth of the tail and distances were obtained rather roughly. The sketches were evidently made on paper on which the right ascensions and declinations had been plotted and several stars inserted beforehand.

Stonyhurst College Observatory. Results of Meteorological and Magnetical Observations. By the Rev. S. J. Perry, S.J., F.R.S., &c., &c. 1884. Manresa Press, Roehampton. 1885. pp. 95.

The contents of this valuable periodical, besides the monthly reports and yearly summary, embrace rainfall, dates of occasional phenomena, sun observations, duration of sunshine, observations of upper clouds (cirrus), agricultural notes, observations of crops, of trees and shrubs, dates of the flowering of plants, the upper glow sunrise and sunset, tables of solar observations on each day during the years 1880—1884, magnetic report, observations of aurora borealis, list of presents received, with an appendix of observations taken at St. Ignatius' College, Malta.

In the introduction it is stated that the total of 281 drawings of the solar surface on 257 days, along with 88 complete measures of the chromosphere, shows that even in our climate useful solar work may be done. . . . The accuracy of the drawings has been tested by comparisons with all the drawings and photographs that were available, and the result is very encouraging for future work of the same description. . . . The spectra of sun spots have been examined on 30 days, and the widening of 200 lines between B and D accurately measured. . . . An excellent 3½-inch achromatic has been constructed by Cooke, of York, and attached to the tube of the large equatorial, in order to facilitate the work with the star spectroscope. The life history of individual spots, with the study of the fainter markings and of the connection between spots and faculæ, have occupied most attention during the past year.

Under the head of Aurora we read, "The remarks above would seem to strengthen what was said last year, "that there is some evidence to show that the aurora and magnetic storms synchronize rather with particular classes of spots than with solar disturbances generally.''

We have received the July number of *Symons' Monthly Meteorological Magazine* (Stanford, Charing Cross. Price 4d., or 5s. per annum, post free), in which is an article by the editor on Scintillation and Weather Forecasting, founded upon papers in *Ciel et Terre*, the Belgian Meteorological Journal, by M. Montigny, well deserving of attention. M. Montigny uses a 3-inch achromatic, and Mr. Symons thinks that his scintillometer might easily be constructed as to become virtually merely an extra eye-piece to such a telescope, and that the cost would probably be covered by from £3 to £5. There is also a notice of the extraordinary fall of rain and snow in Vienna, May 15, 1885, when 5·99, or close upon *six inches* fell in 24 hours. On an average the rainfall of Vienna is 23·43 inches (rather less than that of London). In the same journal is an extract from *The Times* about the deep shaft which is being sunk by the German Government near Schladebach (between Merseburg and Leipzig) which at the beginning of this year had reached the unprecedented depth of 4,567 feet,* at which the temperature was 49° Cent., or 120° Fahr. Supposing a regular increase at this rate, the boiling point of water would be reached at a depth of 9,843 feet, or nearly two miles; and at 47 miles the melting point of platinum would be reached.

Annals of the Astronomical Observatory of Harvard College, Vol. XIV. Part II. Observations with the Meridian Photometer during the years 1879-82. By Edward C. Pickering, Director, aided by Arthur Searle and Oliver C. Wendell, assistants in the Observatory. Cambridge: John Wilson & Son. University Press. 1885.

The contents of the chapters of this concluding part of Professor Pickering's great work are—VII. Ancient Catalogues: Ptolemy, Sufi, and Ulugh Beg. VIII. Catalogues of Sir William and Sir John Herschel. IX. Modern Catalogues: Struve, Uranometria Nova, Heis, Darchrumsterung, Behrmann, Uranometria Argentina, Houzeau, Professor Rogers, Flammarion, Seidel, Wolff, Peirce. X. Additional Series of Modern Observations: Argelander, Bonn Observations, Chandler, Scales of Herschel and Pritchard, Ricketts, Westphal, Zöllner. XI. Additional Photometric Observations. XII. Discordant Observations. XIII. Suspected Variables. XIV. Distribution of Stars: Comparison with Theory. XV. Errors of Catalogues. XVI. Supplementary Results.

Professor Pickering remarks (p. 405) that the relative brightness of the most conspicuous fixed stars cannot be learned from catalogues in which they are designated only as of the first and second magnitudes. But the principal series of photometric measures hitherto published, as well as the accurate estimates made by the Herschels and others, enable us to arrange these stars satisfactorily in the order of their brightness. Table LXVII. contains such an arrangement, which includes the brighter stars of the southern hemisphere. Those who are not in possession of this work may be glad of the following transcript of two columns of this table, which also contains the original results by Sir John Herschel, Seidel, Wolff, Gould, Pritchard, and the Harvard measures and estimates, &c.:—

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* The boring at Sperenberg, near Berlin, reached the depth of 4,172 feet (*Physics of the Earth's Crust*, by the Rev. Osmond Fisher, M.A., F.G.S., &c.).

No.	Design.	Mag.
1	α Can. Maj.	—1·4
2	α Argûs	—0·8
3	α Centauri	—0·1
4	α Boötis	+0·1
5	α Aurigæ	0·2
6	β Orionis	0·2
7	α Lyræ	0·4
8	α Can. Min.	0·6
9	α Eridani	0·6
10	α Orionis	0·8
11	β Centauri	0·8
12	α Crucis	0·9
13	α Tauri	1·0
14	α Aquilæ	1·0
15	α Scorpii	1·1
16	α Virginis	1·2
17	β Gemin.	1·2
18	α Pis. Aus.	1·3
19	α Leonis	1·4
20	β Crucis	1·5
21	α Cygni	1·6
22	ε Can. Maj.	1·6
23	α Gemin.	1·6
24	γ Orionis	1·8
25	α Gruis	1·8
26	ε Orionis	1·8
27	γ Crucis	1·8
28	ζ Orionis	1·8
29	β Tauri	1·9
30	η Urs. Maj.	1·9
31	λ Scorpii	1·9
32	β Argûs	1·9
33	ε Urs. Maj.	1·9
34	α Urs. Maj.	2·0
35	α Persei	2·0
36	γ Argûs	2·0
37	β Aurigæ	2·0
38	ε Argûs	2·0
39	δ Can. Maj.	2·0
40	θ Scorpii	2·1
41	θ Centauri	2·1
42	α Tri. Aus.	2·1
43	α Pavonis	2·1
44	α Androm.	2·1
45	α Urs. Min.	2·1
46	γ Gemin.	2·1
47	β Can. Maj.	2·2
48	α Hydræ	2·2
49	α Arietis	2·2
50	ζ Urs. Maj.	2·2

Four of the stars named in this table have two components, the fainter of which is sufficiently bright to affect the estimate made of the combined light of both. The two components of α Centauri have been photometrically compared by Gill (Heliometer Determinations of Stellar Parallax in the Southern Hemisphere, page 5, note). The observed difference in magnitude was 1·25; hence the separate magnitudes of the two components will be 0·2 and 1·4, by the formula given on page 90. The remaining cases are those of α Geminorum, ζ Ursæ Majoris, and ζ Orionis, in which the corrections in magnitude required to reduce the brightness of both components combined to that of the principal component alone are respectively + 0·4, + 0·2, and + 0·1.

It will be seen that the magnitude of Sirius exceeds that which has before been assigned to it, the scale being that in which a given magnitude is 2·512 (log. of which is ·4) times brighter than the magnitude below it, the difference of magnitude between Sirius and a first magnitude star being — 1·4 + 1 = 2·4. 2·4 × ·4 = ·96 = log. 9·1, so that Sirius is then nine times brighter than a first magnitude star. In the same manner α Argûs (Canopus) is found to be 5·2 times, and α Centauri 2·8 times brighter than a star of the first magnitude. Sir John Herschel (*Outlines, &c.*), considered the light of Sirius to be four times that of α Centauri. By this table it is 3·3 times greater than α Centauri. Of the lowest magnitudes, 2·2 in the table, it will take three such stars to equal the light of a first magnitude star, and 13·9, or close upon 14, to equal Sirius. The only stars, according to the table, of precisely the first magnitude are *a* Tauri and *a* Aquilæ. Some of Professor Pritchard's photometric determinations were given in Vol. xxii., pages 122–24, of the *Astronomical Register*. Some of them nearly agree with the Harvard photometry; others differ from under a tenth to, in some instances, two or three tenths of a magnitude, excepting Sirius, which is ·45 of a magnitude less than the H.P.

During the progress of the work done with the meridian photometer, Prof. Pickering says observations were made of various objects belonging to the solar system, the situation and brightness of which brought them within reach of the instrument. The objects thus observed were Mars, Vesta, Jupiter, and its third satellite; Saturn, Uranus, and Neptune. We abridge the results from Table LXIX.

Object.	Year.	No. of Obs.	Mag. at Mean Opposition.	Residuals.
Mars	1880	12	−1·29	·20
Vesta	1880	12	+6·47	·22
Jupiter	1880	16	−1·28	·35
Satellite III.	1880	5	+4·68	·18
Saturn	1879	3	+1·67	·33
Uranus	1880	5	+5·56	·15
Neptune	1880	1	+7·96	—

The last column contains the numerical mean of the differences between the separate observations and their mean. The discs presented by some of the objects observed, and the faintness of others, made most of the observations most difficult, and the results more discordant than was usual in the work of the meridian photometer.

Many stars belonging to the group of the Pleiades were observed with the meridian photometer. The results are given in Table LXIV., which we give in an abridged form. The designations are according to Bessel. The last column is the probable error:—

Desig.	Name.	R.A. 1880.	Dec. 1880.	Measures. No. of Obs.	Mag.	P.E.
		h. m.	° ′			
16*g*	Celœne ...	3 37·7	+23·55	6	5·23	·04
17*b*	Electra ...	37·8	23·44	9	3·82	·06
18*m*	— ...	38·0	24·28	6	5·63	·08
19*e*	Taygete ...	38·1	24· 5	9	4·44	·06
20*c*	Maia ...	38·7	24· 0	9	3·98	·03
21*k*	Asterope ...	38·8	24·11	6	5·71	·07
22 *l*	— ...	38·9	24· 9	6	6·28	·02
23*d*	Merope ...	39·2	23·34	9	4·22	·05
10	— ...	39·3	23·53	4	6·96	·02
12	— ...	39·9	24· 9	5	6·58	·03
19	— ...	40·3	23·26	6	6·69	·03
20	— ...	40·3	24·13	3	7·29	·10
22	— ...	40·3	23·33	6	6·74	·08
24	— ...	40·4	23·56	5	6·46	·07
25*η*	Alcyone ...	40·4	23·44	8	3·00	·04
28	— ...	41·3	23· 4	5	5·50	·05
29	— ...	41·4	23·59	4	6·56	·05
26*s*	— ...	41·8	23·30	5	6·36	·10
27*f*	Atlas ...	42·0	23·41	8	3·77	·04

Desig.	Name.	R.A. 1880. h. m.	Dec. 1880. ° ′	Measures. No. of Obs.	Measures. Mag.	P.E.
28*h*	Pleione ...	42·1	23·47	5	5·12	·05
32	— ...	42·2	24· 1	5	6·30	·05
34	— ...	42·6	23·21	5	6·10	·05
37	— ...	42·8	23·59	4	7·06	·10
38	— ...	42·9	23·29	5	6·66	·05
39	— ...	43·3	24· 8	4	7·08	·01
40	— ...	43·8	23·36	4	6·64	·09

We congratulate Professor Pickering and his assistants on the completion of this extensive and important work.

Publications of the Cincinnati Observatory. Observations of the Comets of 1883. By H. C. Wilson. A.M., astronomer *pro tem.* Published under the direction of J. G. Porter, A.M., astronomer. Cincinnati, 1885. pp. vi. and 29.

The appearance of the great comet of 1881 directed the attention of the astronomers of this Observatory, which had previously been given almost exclusively to double-star work, to the inviting and important field of cometary research. The results of the observations of the comets of 1881 and 1882 have already appeared in publication No. 7 of the Cincinnati Observatory. The present publication embodies the results of observations on comets 1883, I. and II., made by Mr. H. C. Wilson, assistant-astronomer. The discussion of the physical observations is based upon the investigations of Dr. Bredichin, in the annals of the Moscow Observatory. Mr. Porter adds: "It is confidently hoped that the present publication will prove a welcome addition to our knowledge of these non-shaped bodies, and materially aid in the solution of the problem concerning their physical constitution."

Comet 1883, II., was discovered by Mr. Brooks on the night of September 1. It was soon identified with that discovered by Pons in 1812, returning, according to prediction, after a 70 years' journey, into the regions beyond the orbit of Neptune. It was observed by Mr. Wilson from September 5, 1883, to February 1, 1884, with the 11-in. equatorial, its finder, and an opera-glass. There are eleven plates of sketches of the comet, very well done, and two others of the study of the tail according to Bredichin's method. Mr. Wilson observes, the lithographer, in reproducing the drawings, has made the fainter parts of the comet much brighter than they really appeared in the telescope, a defect, we fancy, not uncommon in figures of comets and nebulæ. This must be regarded as a very valuable publication. Another comet of long period has been carefully studied under favourable circumstances, and these observations cannot fail to be useful.

After February 1, the comet was too far south to be seen at Cincinnati, but was followed by observers in the southern hemisphere, until the middle of the summer. The increase in brightness as it approached the earth and sun was very irregular, and not in accordance with the theoretical law of the light of planets. The testimony of many observers goes to show that there were sudden outbursts of light, corresponding to extraordinary discharges of matter from the nucleus. There were also marked changes in the form and relative brightness of the parts of the head.

The *Bulletin Astronomique* (published under the auspices of the Paris Observatory) for March has, as its *pièce de résistance*, an elaborate mathematical paper by M. Radau on the Elimination of the nodes in the problem of three bodies, that is to say, the reduction of the equations by which one is enabled to refer the movement to one or several planes in the system while obtaining the absolute positions of these planes. M. Schulhof contributes the first portion of a memoir on the orbit of Comet 1873, VII., in which he examines the identity of this body with Comet 1818, I. There are also Observations of Comets Fabry and Brooks, made at the Observatory of Lyons by M. Gonnessiat. The remaining half of the present number is occupied with reviews of recent astronomical publications.

Mr. Gill, in a letter to Admiral Mouchez contained in the April number of the same publication urges the adoption of a plan that could be accomplished in a limited time, which shall comprise not merely photographs of the stars, but also their right ascensions and declinations, with a view to the publication of a Catalogue reduced to some common equinox. He also expresses himself strongly in favour of a plan which shall have for its immediate object the formation of a complete photographic atlas of the heavens on a scale sufficiently large to show the magnitudes and relative positions of the stars. Mr. Gill declares his willingness to co-operate in carrying out either of these plans. The April number contains a second instalment of M. Schulhof's paper on the orbit of Comet 1873, VII.

To avoid the danger of probable loss, Professor Pickering has collected from various sources, generally the local newspapers, the results of the determination of certain mountain heights in New York, New Hampshire, and Vermont.

A new Form of Polarimeter. By Professor E. C. Pickering. (Reprinted from the *Proceedings of the American Academy of Arts and Sciences*).

In this paper the author points out that the advantages of his own previously devised polarimeter and that of Arago appear to be combined in the following modification of the former instrument. The rectangular aperture was replaced by a series of metallic bars separated by intervals exactly equal to their width. The double-image prism is then placed at such a distance that the separation of the images shall equal the width of the bars, so that the two images of the intervals shall be exactly in contact. The bands will then disappear, and the field will be perfectly uniform when the Nicol is turned, so that the two images have equal intensity. A slight movement of the Nicol will render the bands alternately light and dark, and the exact point of disappearance may be determined with great precision. The bands were made by cementing a piece of tinfoil to a plate of glass, ruling a series of parallel lines upon it, and removing the foil from the space between the second and third, and the fourth and fifth lines, &c. The arrangement appears to be very ingenious, and has, moreover, the great merit of simplicity.

Observatoire Royal de Bruxelles. Exposition Critique de la Méthode de Wronski pour la Résolution des problèmes de Mécanique Céleste. Par Ch. Lagrange, Astronome adjoint à l'Observatoire Royal de Bruxelles. Première partie. Bruxelles : F. Hayez. 1882. pp. 70.

M. Lagrange describes Wronski as a person of extraordinary genius, who attacked almost all branches of human knowledge, but whose impetuosity renders it necessary to be on one's guard with him. His method for the solution of problems of celestial mechanics is scattered over several works, especially the *Prolegomènes du Messianisme*, and the *Réforme des Mathématiques* (1847). A peculiar terminology, the constant employment of philosophical considerations, a large number of lacunæ, leaving to the reader the making of calculations sometimes of great complexity; and above all this, the non-enunciation of the principle which is the key of the whole system, have been the cause that a method, in itself well worth being known, has remained in obscurity. In this, the first part of his work, M. Lagrange has explained the dynamical relations of the problem. The second part will contain the analytical methods proposed by Wronski to effect the integrations arising in the first part.

"In the method of Wronski, as in the ordinary method of the *variation of arbitrary constants*, it is proposed to find in function of the time the parameters of a variable conic upon which the body, whose motion is examined, may be regarded as moving at each moment, and the equation of the real trajectory is obtained by replacing in the equation of the conic the constant parameters by their functions of the time. As we here present it, it differs from the ordinary method, principally: 1. By a greater generality; that is to say, his formulæ, true for any trajectory, establish relations concerning the general dynamics of a material point, and comprise, therefore, celestial mechanics as a particular case. 2. By the introduction of new variable parameters, especially the mean velocity, w, between the extreme velocities on the variable conic; of the parameter p of that conic; and above all, of the central mass under the action of which it is described. 3. By a new choice of co-ordinates, and principally by the consideration of a fixed line in the plane of the variable orbit."

It is fortunate that this portion of the writings of Wronski has at last found so able an expositor as M. Lagrange, who has also noted cases where the author has fallen into error and self-contradiction.

THE COMPANION OF SIRIUS.

Professor Young, of the Princeton Observatory, gives the following mean results of observations made there with the 23-inch refractor, from 1883·105 to 1886·072. A power of 460 was usually employed:—

Mean position angle	1884·273	36°·30
" " "	1885·112	34°·06
" " "	1886·047	29°·77
Mean distance	1884·270	8″·70
" "	1885·089	8″·09
" "	1886·049	7″·59

For the position angle 89 observations were made, and 121 for distance. It will be remembered that, at the time of the discovery of the companion in 1862, Bond's observations gave, for its position angle, 84°·5, and for distance 10″·1.

The June number of the *Revista do Observatorio* is mainly devoted to meteorology and seismology. From it we learn that Professor Peters has deduced the following positions of 61 Cygni:—

	1886·0	1887·0	1890·0
Angle of Position ...	120° 7′	120° 31′	120° 55′
Distance	20″·71	20″·81	20″·92.

BROOKS' COMET c, 1886.

From observations made on June 3, 30, and July 1, Dr. S. Oppenheim has obtained the following elliptic elements :—

T = June 6·815958 M. T. Berlin.
ω = 176° 50′ 13″·3
☊ = 52 5 56 ·2
i = 13 23 48 ·0
log q = 0·133357
log e = 9·836962
log a = 0·637824
Period = 9·05 years. *Astron. Nach.*, No. 2739.

SATURN'S RING.

Oct. 20. Major axis of ring = 42″·13
Minor „ = 16″·23
Inclination of earth to plane of ring = 22° 39′·8 S.
Mean obliquity of the ecliptic, Oct. 17 = 23° 27′ 14″·31
Apparent „ „ „ „ = 23 27 6 ·15
Semidiameter of the sun = 16 5 ·2
Parallax of the sun = 8 ·89
Sirius, Aug., 1886.

THE PLANETS FOR OCTOBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	12 41 23	S. 3 20	4″·8	0 0·8
	9th	13 30 8	S. 9 15½	4″·8	0 18·0
	17th	14 17 20	S.14 32½	4″·8	0 33·6
	25th	15 3 59	S.19 0½	5″·2	0 48·7
Venus ...	1st	11 38 49	N.52 23	10″·2	22 54·5
	9th	12 15 27	S. 0 4	10″·0	22 59·6
	17th	12 52 10	S. 4 2	10″·0	23 4·7
	25th	13 29 17	S. 7 56	9″·8	23 10·3
Saturn ...	1st	7 32 59	N.21 25	16″·2	18 49·4
	9th	7 34 44	N.21 21½	16″·4	18 19·6
	17th	7 36 11	N.21 19	16″·6	17 49·4
	25th	7 36 49	N.22 18	17″·0	17 18·8
Neptune ...	4th	3 42 40	N.17 54½		14 47·8
	24th	3 40 55	N.17 48		13 27·4

Mercury is badly situated for observation, setting a few minutes after the sun at the beginning of the month, the interval increasing to about half an hour after sunset.

Venus rises about an hour and a half before the sun on the 1st, the interval decreasing.

Saturn rises an hour and a quarter before midnight, and afterwards earlier each night.

ASTRONOMICAL OCCURRENCES FOR OCTOBER, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Fri	1		Sidereal Time at Mean Noon 12h. 40m. 34·02s.			10 16·8
Sat	2		Sun's Meridian Passage 10m. 39·74s. before Mean Noon			10 12·9
Sun	3	14	Conjunction of Jupiter and Mercury 0° 22′ S.			10 9·0
Mon	4	10 33	☽ Moon's First Quarter	Jupiter's		10 5·1
Tues	5					10 1·1
Wed	6	10 33 11 42	Occultation of B.A.C. 7097 (6) Reappearance of ditto	Satellites		9 57·2
Thur	7					9 53·3
Fri	8	10 29	Near approach of e^2 Aquarii (5½)	invisible		9 49·3
Sat	9	5	Conjunction of Jupiter and Sun			9 45·4
Sun	10		Saturn's Ring: Major axis=41″·37 Minor axis=16″·00	through		9 41·5
Mon	11					9 37·5
Tues	12	15 23 11 58 12 15	○ Full Moon Occultation of *f* Piscium (5½) Reappearance of ditto	his		9 33·6
Wed	13		Sidereal Time at Mean Noon 13h. 27m. 52·64s.	nearness		9 29·6
Thur	14	7 1 0	Occultation reappearance of μ Ceti (4) Conjunction of Uranus and Venus 0° 52′ N.	to the		9 25·7
Fri	15	3	Saturn at quadrature with the Sun Illuminated portion of disc of Venus=0·978 Illuminated portion of disc of Mars=0·936	Sun.		9 21·8
Sat	16	7 36 7 27 7 25 8 4 7 36 8 20	Occultation reappearance θ^2 Tauri (4½) Occultation reappearance of θ^1 Tauri (4½) Occultation of 80 Tauri (6) Reappearance of ditto Occultation of 81 Tauri (5½) Reappearance of ditto			9 17·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Sat	16	8 5 8 5 8 59 10 37	Near approach of B.A.C. 1391 Occultation of 85 Tauri (6) Reappearance of ditto Near approach of α Tauri (1)			—
Sun	17	8 14 9 9 9 17	Occultation reappearance of 111 Tauri (5½) Occultation of 117 Tauri (6) Reappearance of ditto	Jupiter's		9 13·9
Mon	18		Sun's Meridian Passage 14m. 47·47s. before Mean Noon			9 9·9
Tues	19	19	Conjunction of Moon and Saturn 3° 16′ N.	Satellites		9 6·0
Wed	20	2 40	☾ Moon's Last Quarter			9 2·1
Thur	21			invisible		8 58·2
Fri	22	13 23 14 6 14 38 14 14 14 49 9	Occultation reappearance of B.A.C. 3538 (6½) Occultation of 44 Leonis (6) Reappearance of ditto Occultation of B.A.C. 3562 (6½) Reappearance of ditto Conjunction of Jupiter and Venus 0° 18′ N.	through his		8 54·2
Sat	23			nearness		8 50·3
Sun	24					8 46·4
Mon	25			to the		8 42·4
Tues	26	19 15 1	● New Moon Conjunction of Moon and Venus 2° 36′ S.			8 38·5
Wed	27			Sun.		8 34·6
Thur	28	1	Conjunction of Moon and Mercury 7° 3′ S.			8 30·4
Fri	29	22	Conjunction of Moon and Mars 6° 5′ S.			8 26·7
Sat	30		Saturn's Ring : Major axis=42″·92 Minor axis=16″·51			8 22·8
Sun	31					8 18·8

THE CANALS ON MARS.

In a note on this subject by M. Perrotin, published in the *Bulletin Astronomique*, III. 324, the author gives an account of observations made by himself and M. Thollon during the recent apparition of the planet. Their chief object was to search for the single or double canals, which had been discovered by M. Schiaparelli, and observed by scarcely any one but himself. The observations were made with an equatorial of 0·38 m. aperture, the powers usually employed being 450 and 560. The first attempts were not encouraging, partly owing to the difficulty of the observations, and partly due to the badness of the seeing. On April 15, however, both observers saw one of the canals situated to the west of the Kaiser Sea, making a communication between that sea and Herschel Strait. Several other of M. Schiaparelli's canals were afterwards recognised. They appear to form a network of lines following arcs of a great circle in all directions, sometimes uniting the seas of the two hemispheres, sometimes merely forming a junction with one another. Some of the lines that have been measured extend 50° or 60° in length. Many of the canals are double, and composed of strictly parallel lines, the distances of which, according to M. Schiaparelli's estimation, vary between 6° and 12°. MM. Perrotin and Thollon also draw attention to some changes which they have observed on certain portions of the planet, particularly in the neighbourhood of the Kaiser Sea.

Books received:—Revista do Observatorio. Rio de Janeiro. 1886.—Investigation of Stellar Photography at Harvard College. By E. C. Pickering. Cambridge, U.S. John Wilson & Son.—Sirius. No. 9. Leipsic. 1886.—L'Astronomie. Par Camille Flammarion. Paris: Gauthier-Villars. 1886.—Astronomische Nachrichten.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 287. NOVEMBER. 1886.

REPORTED CONTINUANCE OF CHANGE IN THE GREAT NEBULA IN ANDROMEDA. Question as to the correctness of those reports.

(From *Astronomische Nachrichten,* No. 2,751.)

Herr N. von Konkoly writes as follows from Ogyalla, under date of the 3rd of October:

"It was announced to me four or five days ago by the observer in my observatory, Dr. Rudolph von Kövesligethy, who is now away on leave, that changes are again taking place in the great nebula in Andromeda. He is at present at Baron Géiza von Podmaniczky's, in Kis Kartal, where recently the Baron's fine private observatory was completed, and where a very fine 8-inch by Merz, mounted by Cooke, has been installed. The Doctor requested me to look to the matter, and possibly confirm the phenomenon. Unfortunately I have at my disposition at present nothing but a 6-inch refractor, as my 10-inch one has been dismounted, in consequence of an alteration being made in the construction of the dome. The state of the air was far from good, and I must confess I could make out nothing.

"This afternoon I received a further communication from the observer, and looked again at the Andromeda nebula; to my astonishment I saw therein a star, without any question, of from the 10th to the 10·5th magnitude, and can but confirm what Dr. Kövesligethy wrote to me, namely, that the star can be seen much better with a Zöllner's spectroscope than by means of an eyepiece, inasmuch as the spectrum detaches itself well from the bright background of the nebula, "As I did not consider it advisable to continue the observations with a large spectroscope, as the state of the air was by no means good, I proceeded forthwith to the telegraph-room of the observatory, to enquire of Herr von Gothard as to the weather, receiving from him the reply, from Herény, that the sky was clear. I therefore requested him to photograph the nebula without delay. He did so, and I hope it will not be long before we receive his report here in black and white."

Letter from Herr Eugene von Gothard, at Herény, dated October 5:

"On the 3rd of October, while occupied with a spectroscopic observation of the ring-nebula in the Lyra, I received a telephonic message from Herr von Konkoly that the new star in the Andromeda nebula had began to become visible. I at once gave up my fruitless observation, and photographed the nebula with the nova, which in point of fact was there. After an exposure of 60 minutes, the nebula was shown as nebulous matter, round in shape and growing continually more condensed towards the centre. It was about 20" in diameter, with a somewhat nebulous star in the middle.

"When the negative had become dry, I compared it with one taken on the 7th of September, 1885. As the two have been taken with the same instrument, the neighbouring stars correspond together perfectly, the new star, too, coincides so completely with that of last year that the two may be held to be identical. Unfortunately the 1885 photograph is by no means as perfect as the recent one, the stars are not absolutely round discs, and this increases the difficulty of their comparison.

"I have also photographed the nebula, in the latter half of August, but the plate is unfortunately so much impaired by several spots, that one can no longer distinguish the stars among each other. With care one can pick out the spot corresponding to the nebula, the condensation—as far as the faulty photograph permits of one judging—is to be seen well, the star, however, is not visible. I remark again that this photograph is not much to be relied on.

"I endeavoured to observe its spectrum, which detaches itself well from the luminous background, but it is not possible to see details.

"A second, taken on October 4, upon a plate sensitised with chynoline-red, showed the star more distinctly still. I have two such takings, side by side, upon a single plate. Time of exposure 40 and 20 minutes. I purpose instituting experiments for photographing the spectrum of the nova, and as my taking of the spectrum of the faint Wolf-Rayet comet was successful, I hope to obtain satisfactory results with respect to the spectrum of the nova as well. But I shall have to wait for a night that is clear and free from vapours."

Letter from Herr J. A. von Bártfay, in Budapest, at the Polytechnic Observatory, dated October 9:

"In a letter dated October 2, Dr. von Kövesligethy writes to me as follows: 'Indications of change are showing themselves again in the Andromeda nebula.' This information reached me on the 3rd of the month, and on the night of the same day I examined the nebula in question with the 5-inch telescope; at the first glance it was seen that the condensation was divided. Steinheil's eyepiece. Power 120.

"The day following, the 4th, and yet better on the 5th, I could indeed distinguish the same star very well, but since no notice thereof had come to hand from other observatories provided with far larger instruments, I did not, at the first moment, think the star could be a new one, but a comparison with drawings did away with all doubt as to the star to which I had directed my attention since the 3rd of the month being the nova, as I had indeed stated in the local papers.

"On the 6th of October, at 10h. 53m., I saw the star for a few moments between clouds, and estimated it to be of the 9-10th magnitude. The sky had become quite clear at 13h. 30m., and I obtained a very fine image with Steinheil's eyepiece. Magnitude = 9. With a deeper eyepiece, power 250, the nova still preserved a stellar appearance, whereas, on the contrary, the other portion was quite indistinct.

"8th October, 8h. 30m., very bright moonlight, cloudy sky. I only see the nova, and observe no further irregularities in the nebula.—12h. 10m. Still much impeded by the moon; the star is of about the 8th magnitude."

Addition by the Editor:

"From the communications of Herr von Konkoly and Herr von Gothard, it does not appear altogether clear whether only the old stellar nucleus, 10-11 magnitude, or the new star of last year has been observed.

"With regard to what Herr von Bârtfay writes, his communication may be said to be a very remarkable one, inasmuch as on the 5th of October, Dr. E. Lamp, with an 8-inch refractor, had seen nothing remarkable in the Andromeda nebula. A permanent clearing-up of the sky last night permitted of a test-examination being effected, during bright moonlight it is true, but this likewise gave a negative result. Neither Dr. Lamp, with the 8-inch refractor, nor I with a 5-inch finder was able to see anything remarkable in the nebula. Moreover, on the same evening telegraphic enquiries were despatched to Potsdam and to Bonn. At an early hour this morning I received a reply from Bonn, to the effect that, with a moderately clear sky, nothing remarkable was seen there, too."

Kiel: Oct. 12, 1886. (Signed) Kr.

LIVERPOOL ASTRONOMICAL SOCIETY.

The first meeting of the sixth session was held on Monday, the 11th October, at the Association Hall. The chair was occupied by the Rev. T. E. Espin, B.A., F.R.A.S. (president). Eighty-one members were elected and ten candidates were proposed. Of the total number 15 were from Liverpool, 24 from other parts of England and Scotland, 9 from Ireland, and 43 from North and South America. Mr. W. H. Davies (secretary) announced the deaths of four members during the past year. A short account of their lives had been prepared, and would be published in the *Journal.*

The President complimented the council on their able management. He thought the long list of members which the Secretary had just read was without a parallel in any scientific society in the world, and likely to prove by far the most important paper of the evening. (Laughter.) To the students of the educational branch he would especially address himself, and he thought he could promise they would never regret their choice of a science. Of all others, astronomy was the most fascinating, and the love of it only increased with its study. As an example of devotion he instanced Sir W. Herschel, who once spent more than forty hours of uninterrupted work in putting the finishing touches on his large speculum, and could not be induced to leave it even for food. It had been said that night work was injurious, but he had never found it so, though, last winter, he spent many nights knee-deep in the snow. Perhaps this immunity was in some measure due to the clearness of the air, since no astronomer would think of uncovering his telescope in foggy or rainy weather. He had headed his address "Stellar Variations: A Chapter in a World's History," and they would be prepared to hear that he regarded variation as a stage of development through which perhaps every star either has passed or must pass. Might not variation be due to stages of axial rotation? Let them suppose in the star's early history a central heat and external vapours in a cooler state; they had then a spectrum of the third type. Let the axial rotation go on increasing, and the temperature also, then there would be a struggle between the hotter and cooler vapours, and we should have an irregular variable like 19 Piscium. Passing to a time when the rotation had quickened, they could imagine the absorption vapours more and more compressed in area; this would give a long-period variable with great variation in its light. Taking R Hydræ as an example, they would find its period in 1785 500 days, but in 1825 it was only 461, and in 1870 it had decreased to 437; in other words, its rotation was now 60 days less. Let them suppose that a time would come when, by increased rotation and temperature, the vapour would no longer have power of cohesion, and would be thrown out irregularly all round the star. At this stage they might expect to find a spectrum merging from III *a* into II, and the light irregularly variable. He forebore to speculate on the origin or the end of such a star, or whether it ever had been or would become a nebula, but these chapters in a star's history were full of interest. In variable stars they might read something of our sun's history. In the geological record, had they any hints as to a past history of great variation? Those strata, each showing a higher period of development, were they impressions of contemporaneous solar development? Would our sun ever lose these vapours of absorption, and stand out as a star of the first type like Sirius, with its present heat increased a hundred-fold? How bright will it then be? and what will be the period of its rotation? Will animal or vegetable life exist under those conditions? Such questions must remain unanswered in our time, and we must be content to observe, to watch, and to record.

In a paper *On the retardation of Encke's comet*, Mr. W. H. S. Monck, M.A., F.R.A.S., accounted for its slower motion by its passing through systems of meteors, rather than by the generally accepted theory of a resisting gaseous medium. Wherever meteors existed, the motion of any celestial body must be retarded by their influence, because a large majority must be coming from an opposite direction. This would be understood if they considered that in walking along a tram-line they would meet with more cars than would catch up to them; not only so, but the momentum would be greater, as their own motion would practically be added to it. The number of meteors flying about space was greater than most people imagined; it had been computed that the earth receives the impact of several millions in the course of a single day. In the case of a solid body like the earth, the shock would be too small to be appreciable; but with a light body like Encke's comet, the effect would be, to seriously retard the motion. Collisions with meteors would also be more frequent nearer the sun, at all events up to the distance that Encke's comet approached it. Astronomers had inferred the presence of a great mass of meteors inside the orbit of Mercury as the only way of explaining certain irregularities in the motion of that planet; and, as both meteors and comets would then be near their perihelion, they would be moving with increased speed, so that the retarding effect would be greater.

Mr. W. F. Denning, F.R.A.S., pointed out that in July and August, when the earth is further from the sun, there are quite double the numbers of meteors that are visible in January and February. This alone would go far to negative Mr. Monck's theory that meteors were more crowded near the sun. It should also be remembered that Encke's comet was not a cohert mass of matter, but a collection of very small bodies. The effect of this collision with meteor streams would therefore be of a very different nature to that in which a solid planetary body like the earth was concerned. A meteor stream would probably pass completely through Encke's comet—itself but a highly condensed group of meteors—without affecting its motion. There would be virtual, but not actual, collision, except perhaps in the case of a very few individual atoms of the opposing systems. A solid body presenting a compact front would receive all the force of the impetus, but in the case of a comet and meteor stream there would only be permeation.

The Secretary read a paper *On the search for Vulcan*, by Mr. T. W. Backhouse, F.R.A.S. It was of great importance that the existence of an intra-Mercurial planet should be determined, and a systematic watch ought to be kept during the period when, according to Leverrier's calculation, it was possible for Vulcan to be in transit across the sun's face. This would entail very little extra trouble now that the society had so many observers in both hemispheres, many of whom were constantly watching the sun in the interest of other branches of astronomical research.

Mr. T. G. Elger, F.R.A.S., addressed some remarks, especially to beginners. In many respects, he said, a study of the moon presented advantages such as no other celestial body could claim. There we saw the actual surface of another world unobscured by clouds or exhalations, and could contemplate the marvellous details of its structure, rather in the spirit of a physical geographer, than of a telescopist. The beginner would soon see that the moon had been nothing like so exhaustively studied as to leave nothing for small telescopes to do. In commencing this study they should first make themselves acquainted with the more obvious characteristics into which its surface is divided, and in this they would be much assisted by Webb's, Mellor's, or other convenient maps. As a rule they would find the majority of objects easier to observe when the sun had just risen on them. At those times the surface was brought into strong relief by shadows, the significance of which they would soon learn to interpret.

Mr. K. J. Tarrant counselled beginners not to be discouraged if they could not at once distinguish objects with small apertures. Nowadays, if a man wrote to say that he had picked up such and such an object, or had split a close double star, they might expect the observation to be rivalled by someone else with a smaller aperture; indeed he sometimes wondered whether imagination had not a great deal to do with it. This kind of thing was very disheartening; and no doubt many a good telescope had been condemned on very insufficient grounds. Of course a great deal depended upon the air. As an example: there was one abnormally clear night last winter when the fifth and sixth stars θ Orionis were quite easy objects; whereas, on the next night, which was apparently quite as clear, they were undistinguishable.

Papers were read *On the classification of variable stars,* by Mr. J. E. Gore, F.R.A.S.; on *Cassini,* by M. C. M. Gaudibert. Other papers have been received—*On the effect of terrestrial heat on the moon,* by Mr. W. H. S. Monck; *On some remarkable sun spots,* by Miss. E. Brown; *On the masses and distances of binary stars,* by Mr. J. E. Gore; *On the lunar crater Mädler,* by Mr. T. G. Elger, and *On interesting facts about binary systems,* by Mr. K. J. Tarrant. Arrangements were made to hold educational classes twice a month, and Mr. J. S. Brown, 274, Upper Parliament Street, was appointed secretary to the branch.

SELENOGRAPHICAL NOTES, NOVEMBER, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Chevallier, Oersted, &c. The plain on the south-west of Atlas includes some objects of a very peculiar type, of which Chevallier and Oersted are notable examples. In the place of a more or less lofty border enclosing an area differing generally in level and hue from the surrounding country, their limits are defined by boundaries wholly inconspicuous save under very oblique light and which, in every respect, resemble ridges curcumscribing spaces of the same uniform grey tint as plain outside. This ridge-like character is more pronounced in the case of Chevallier than in that of its companion, Oersted, on the east. If he chooses a time when the evening terminator intersects the neighbouring Endymion, the observer will note as one of the most conspicuous objects near Atlas the large crater Atlas A with a bright little companion close to its north-east wall. Running in a southerly direction from the western side of A, he will remark, at this phase, the well-defined ridge forming the eastern border of Chevallier, the northern side being marked out by a second ridge, also originating on the west of A, which throws out a long winding branch trending northwards towards Endymion. The western border of the formation consists of two curved ridges, one curving northwards from near the southern end of the eastern side, and the other southwards from the end of the north ridge, the two overlapping at a point a little west of the bright crater *b* on the floor. In Schmidt's map, Sect. xiv., the floor of Chevallier is shown as devoid of detail with the exception of *b*, nor have I ever seen any markings on its apparently even interior. Neison, however, mentions several low hills. These features should be looked for, and their position with respect to *b* noted.

Oersted is a less anomalous formation than its neighbour, the wall being continuous and apparently of greater average altitude, though the south-east section can only be traced when it is close to the terminator. There is a rhomboidal-shaped enclosure connected with the north-eastern side of Oersted which Schmidt draws far too prominently as a circular ring-plain while Neison barely indicates it. At the point where the western wall of this object joins the north wall of Oersted, there is a bright crater, and aligning with the latter and the south end of Atlas A are two more bright craters standing on light areas. An ill-defined glimmering streak connects these features with the crater on the wall. Schmidt saw two minute craterlets on the floor of Oersted, in addition to the large craterpit, close to the north-east wall.

Southern Section of the Sirsalis Cleft. The following observations relating to this portion of the great cleft have been recently forwarded to me by M. E. Stuyvaert, of Brussels. They were made by him on 20th November, 1885, 11h. 10m. Brussels M.T., with a power of 260 on the 15 inch equatorial attached to the observatory, the east longitude of the morning terminator being 76° 25′. "The Sirsalis cleft traverses the N.W. rampart, and the bottom of De Vico *a*. Beyond the S.E. rampart, which it does not cut through, it runs on the east side of the ridge De Vico γ where it bifurcates: the small branch bending towards the south-east; the other, much more delicate, follows the ridge De Vico γ, and ultimately widens south of Byrgius *e* into a valley.

The branch which traverses Byrgius was not observed. Before reaching the ridge De Vico γ, a branch is thrown out from the cleft trending towards the north-east, and continues beyond the chain of mountains ζ α: it is the cleft η. To the east of this chain of mountains the cleft η is crossed by another cleft coming from the south, which extends in a northerly direction as far as a, a little crater S.W. of Crüger." These interesting observations include some details not hitherto recorded, as, for instance, the bifurcation of the cleft at the ridge De Vico α and the northerly extension of cleft η to the crater near Crüger. The branch extending in a north-easterly direction beyond the mountains ζ α is, under favourable conditions of libration, one of the most prominent clefts on the moon.

Kempston Beds, 19th October, 1886.

SIDEREAL NOTES FOR NOVEMBER, 1886.

By J. E. Gore, F.R.A.S.

The following variable stars may be well observed in November:—

1. 100 Piscium. R.A. 1h. 28m. 39s., N. 11° 58′ (1885·0). Announced by M. Borrelly in 1885 as variable from 7 mag. to 9 mag. It was rated 8, 8½, 9, and 10 mag. by Lalande. My observations (September—December, 1885) confirm the variability, and show a maximum (7·2 mag.) about November 30, 1885. This star deserves careful attention, as its period has not yet been determined. It lies closely *n.p.* the 5½ mag. star π Piscium.

2. o (Mira) Ceti. R.A. 2h. 13m. 35s., S. 3° 29′·7 (1886·0). This famous variable will be very favourably situated for observation this year when at its maximum, and observers will have a good opportunity of watching its wonderful light changes. Computing by Schönfeld's formulæ of sines, I find that a maximum will be due this year on November 13, but judging from the date of the last maximum (about December 22, 1885, according to my observations) the star may possibly not reach its maximum brightness till some days later than the computed epoch. The maximum of December last was an unusually faint one (not much above 5 mag.). At the preceding maximum, February 4, 1885, Mira was about equal in brightness to α Ceti (2·68 mag. in "H.P."). The star usually becomes visible to the naked eye (6 mag.) about three weeks or a month before the

maximum. The spectrum is a very remarkable one of Secchi's third type, with extraordinarily large and black bands.

3. *r* Ursæ Majoris. R.A. 12h. 31m. 12s., N. 60° 7′ (1886·0). The magnitude at different maxima varies from 7·0 to 8·3, and at minimum the star descends to the 13th magnitude. The mean period is about 255·6 days, and a maximum will be due this year about November 22.

The following "suspected" variable stars are well situated for observation in November, and are deserving of especial observation:—

1. Lalande 44782 Piscium. R.A. 22h. 47m. 38s., N. 1° 12′4 (1880·0). Rated 7 and 7½ mag. by Lalande; 8 by Bessel; 6½ Lamont; 8 Argelander; 6–7 Heis; but found much below 7 mag. at Cordoba. Dr. Gould thinks it "not improbably variable" (*U. A.*, p. 40). On September 17, 1884, I estimated it 7·0; November 7, 7·1; September 8, 1885, 7·0; and October 3, 7·1. It lies about 50′ north of, and a little *preceding* ı Piscium.

2. Lalande 45582 = 303 Bode, Aquarii. R.A. 23h. 11m. 24s., S. 12° 22′ (1880). In 1869 Schmidt called attention to this star as probably variable, and estimated it only 7 mag., although rated 6 mag. by Argelander and Heis. It was estimated 6·6 at Cordoba in 1872 and 1874, and 7 on the meridian in 1877. It is 6·29 in the Harvard Photometry.

3. Birmingham 637 Pegasi. R.A. 23h. 14m. 15s., N. 22° 26′·1 (1880·0). Rated 8·5 by Lamont, and 7 by Bessel. Birmingham's observations (1873—1875) vary from 5 to 7·5; Webb found it 8 mag., December 12, 1873. My observations (1878—1883) record the star as 7–7½ mag., with only slight fluctuations of brightness.

4. Lalande 45980 Aquarii. R.A. 23h. 23m. 18s., S. 5° 11′ (1880·0). 7 mag. Lalande, Piazzi, and Bessel; 6–5 Heis; 7·1 Johnson, 1858; estimated 6·3 at Cordoba, 1872 and 1874, but only 7¼ on the meridian, November, 1877. Dr. Gould says, "Its variability seems probable." On September 30, 1885, I estimated it 6·3.

5. Lalande 46090 Aquarii. R.A. 23h. 26m. 12s., S. 11° 39′·7 (1880·0). 6 mag. Argelander and Heis, but found by Schmidt to be invisible to the naked eye on July 9 and 10, 1878. It was rated 7 mag. by Bessel, and only 8-9 by Lamont. The Cordoba estimates are 6·7, 6·8 and 7·1. It was measured 6·67 at Harvard. On Sept. 21, 1879, I estimated it 6½ mag.

6. 3 Ceti. R.A. 23h. 58m. 21s., S. 11° 10′·5 (1880·0). 6 mag. Heis; 5·22 H.P. Dr. Gould says, "The Cordoba estimates of magnitude of this star range through the whole interval, from

4·9 to 5·9, leaving in my mind small doubt of the variability of its light." On Feb. 4, 1885, I estimated it 5·4; Dec. 5, 1885, 5·1.

7. Lalande 2598 Ceti. R.A. 1h. 19m. 46s., S. 4° 35′ (1880). 6½ Lalande; 8 Bessel. The Cordoba estimates vary from 6·5 to 7·8, and Dr. Gould says, "This is certainly variable." I estimated it, Nov. 13, 1884, 6·7 mag.; Dec. 21, 6·7; Nov. 28, 1885, 6·7.

8. Lalande 2798 Ceti. R.A. 1h. 25m. 29s., S. 7° 20′·2 (1880·0). 6·9 mag. Gould, who marks it "var. (?)" in the *Uranometria Argentina*, but gives no particulars. On Nov. 13, 1884, I found the star only 8 mag.; Dec. 21, below 8 mag.; Nov. 28, 1885, about 8 mag. If the Cordoba estimate of 6·9 is correct, the star must certainly be variable.

The following double star is deserving of further attention, as there is some reason to suppose that the companion may be variable:

Struve 547 Eridani. R.A. 4h. 20m. 3s., S. 1° 39′·3 (1885·0). The companion was rated 11·5 by Struve in 1829 and 1832. It was missed by Dembowski in 1865, by Burnham in 1873 and 1876, and by Gledhill in 1879. It was, however, measured by Mädler in 1845, by Burnham in 1877, 1879, 1880, and 1881, and by Gledhill in 1880 (15°·8 : 2″·4).

Measures of the following interesting binary stars are desirable:

1. Struve 2107. R.A. 16h. 47m. 6s., N. 28° 52′ (1880·0) 6·5, 8 : 212·3 : 0″·99, 1875·61 (Dunér).

2. τ Cygni. R.A. 21h. 10m., N. 37° 32′ (1880·0), 3·7, 7·8 : 116°·3 : 1″·10 (Tarrant 1885·90). Position angle decreasing. Short period.

Minima of Algol may be observed on Nov. 16, about 9h. 34m. p.m., and on Nov. 19 at 6h. 23m. p.m.

THE OBSERVATORY OF NICE, AND ASTRONOMY IN FRANCE.[*]

We have been visiting the workshops of the celebrated Eiffel, and seen the largest revolving dome that has ever been constructed. This floating marvel is not less than 73½ feet in its interior diameter, which is 6½ feet more than that of the Pantheon. It weighs 93½ tons, is made of iron, composed of 620 plates, fastened together by 55,000 rivets. It is simply placed on water, and floats like a vessel so lightly that the hand of a Parisienne can turn it with the aid of a small windlass. It makes a complete revolution in less than four minutes. A slightly strong wind even is enough to set it in motion. This dome is destined for the Observatory of Nice. The result obtained by the daring constructor, to whom is due the project of the tower of 980 feet high for the exhibition of 1889, is as marvellous as unexpected. The engineers, invited by the minister of public works to examine the competition projects for the dome of 65½ feet, for which the Observatory is still waiting—and, unfortunately, will still have to wait a long time—were opposed to the new and original idea of supporting an astronomical dome on water. It was discussed, nevertheless, and rejected by four voices against three. It had, however, a powerful advocate in Admiral Mouchez, director of the Observatory, as enlightened a judge as he is independent.

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[*] Article in L'Astronomie for June, 1885, by M. Flammarion, abridged.

M. Charles Garnier, the architect of the Opera House, who had completed the building of the Observatory at Nice, due to the scientific generosity, approached by nothing like it in France, of M. Bischoffsheim, was one of the committee of the seven, and one of the three who supported the Eiffel project. Having to place a dome upon the magnificent Egyptian base which commands the Mount Gros at Nice, Phidias induced Mæcenas to choose the floating dome in preference to all the old systems. If it had been a question of a State establishment, of the Observatory of Paris, for instance, it would have been necessary to follow the opinion of the official commission, and in place of this aerial dome, which is a mere plaything in the hand of the observer or of his assistant, there would have been constructed a massive fabric, like that of the terrace of the Observatory, like that dome of the Eastern tower, made in the days of Arago, which has wearied the patience of the few astronomers who have faced themselves to observe the heavens under that heavy carapace. . . .

As we have said, the novelty of this construction consists in the application of the principle of Archimedes: every body plunged in water loses a portion of its weight equivalent to the water which it displaces. The dome weighs 209,400 lbs.: a circular vessel has therefore been constructed to support it, which can hold more than 20,910 gallons = 209,100 lbs. of water. The base of the dome sinks to a certain extent in this water, and floats like a boat. This circular vessel, of the same diameter as the dome in the interior, is 5 feet high and 4 feet broad. . . If required, in order to repair the vessel for instance, the dome can turn on rollers.

Water is not used for the vessel, as everyone knows that it freezes at zero, and it would be imprudent, even on the mountain of Nice, to run the risk of finding the dome immovable precisely during the fine nights of winter. M. Eiffel has chosen a solution of chloride of magnesium, which does not freeze, except at 40° below zero, a degree of cold without example at Nice, or Paris, or anywhere in the habitable world. It has been objected that chloride of magnesium might attack the metals; but M. Pictet has used it for ten years in his freezing apparatus, and has never noticed any deterioration.

This fine project is now realized. The dome is about to be dismounted, and transported to the base which awaits it at Nice. It will cover a colossal equatorial, 59 feet in length, the object glass of which has been successfully made by M. Feil, and will be worked to its highest right curvature by the skilful care of the MM. Henry Brothers of the Paris Observatory, celebrated both for their astronomical discoveries, and their optical labours, which have long made them the rivals and successors of Foucault.

There, on the mountain that commands the aerial heights of the city of noise and of pleasures, in the bosom of the solemn silence of the rocks and woods, in face of one of the finest tableaux of nature that can be seen, between the Mediterranean with its resplendent mirror, and the Alps, crowned with their eternal snows, the astronomers of the Observatory of Nice will continue and develop the ingenious researches, and the fine discoveries which have already signalized their accession. Already, with the first instruments placed at their disposal by a scientific generosity which cannot be sufficiently praised, the double stars, planets, comets have been minutely studied, examined, and measured by M. Perrotin, the Director of the Observatory, and his able assistant, M. Charlois; already too, by the aid of a marvellous apparatus, entirely of his own invention, M. Thollon has been able to penetrate into the mystery of the chemical constitution of the sun, by obtaining a solar spectrum 49 feet in length, on which he has counted and measured more than 10,000 lines, a large number of which belong to the atmosphere of the earth. By the comparison of those solar and terrestrial lines, valuable data are obtained, not only relating to the constitution of the sun, but that of our own planet. For its situation, for the ability of the men of science who labour there, for the excellence of its instruments, the observatory of Nice may henceforth be considered as amongst the first in the whole world. . . .

In this sanctuary of Urania there reigns a spirit of scientific liberalism, of loyal brotherhood, of sincerity and amenity, which we rarely meet with in observatories. We breathe there an air of liberty which harmonizes well with the superiority of the light gained on those heights. All astronomers who visit the Observatory of Nice, perceive it at once, and cannot help congratulating the Director on account of it. Le Verrier was a great genius, but it must be confessed that the tradition he has left at the Observatory of Paris is in other respects deplorable.

When will *savants* comprehend that official places signify *nothing*, absolutely nothing, and that the *only* worth of man consists in his personal worth? The man with a heart is fortunate in meeting on his route with a Republic of workers such as that of the Nice Observatory; and though the modesty of these explorers of the celestial mysteries may be hurt, he thinks it just and also opportune to signalize its existence. All true friends of science will admire the generosity towards science of the fortunate deputy of the Maritime Alps, and will felicitate him on the manner in which his work has been carried out. But will the Observatory of Mr. Bischoffsheim preserve its independence in the future?

French education, since the centralization effected by Louis XIV., is become such that office and place dominate over most minds: so much so, that in general the man the most gifted, instead of being content with being some *one*, thinks it is better to be some *thing*. It appears that the official consecration of a merit is a title of guarantee. Nevertheless, at bottom, everyone knows that it is nothing that one meets at every step high and powerful functionaries devoid of all personal worth, veritable crustacea of civilization, and beside them are learned men, tradesmen, literary persons, artists of far greater skill, and who possess the merit of being free from all official connection. Do we inquire *what* were Christopher Columbus, Copernicus, Newton, Pascal, Leibnitz, Herschel; whether they were academicians, or wore gold lace?

The Observatory of Nice is entirely due to the private initiative, the noble generosity, of a friend of science and of progress. M. Bischoffsheim has devoted to it several millions (of francs) in the purchase of the land for the buildings, the instruments, and the *personnel.* He it is who meets *all* the expenses: his intention is certainly to render *the future* as *sure* as the present. So far he has done everything apart from the State. Only three years have elapsed since work was possible, and the results are already wonderful, whilst we might, if we wished, set down columns of figures representing hundreds of thousands, nay even millions of francs, raised by our ever-increasing taxes, contributed to the establishments of the State, and which bring nothing, *absolutely nothing,* to science. This fine example of private initiative, common in the United States and in England, but so rare as to be unique in France, will it inaugurate for our country an era of emulation and progress? Are we about to cease remaining asleep on the pillow of the State? Are we about to give evidence of existence, of personality, of independence, of energy, like our models in the New World and England? It is to be feared not.

It is said that M. Bischoffsheim himself does not adequately appreciate the character of his work, and that he has not in view to set this great example to France, and that as soon as the Observatory is completely furnished, he will do like the rest of the world, and surrender his creation to that great and gently flowing river, the State. It is reported that it is his intention to take the opportunity of the inauguration, which is to be celebrated in October next, to offer the keys of the Observatory to the Bureau des Longitudes! Such a thing would be infinitely to be regretted, in every point of view.

REVIEWS.

Nouveau Dessin du Spectre Solaire. Par M. L. Thollon (*Bulletin Astronomique*, III. 330).

In 1879 M. Thollon presented to the French Academy a drawing of the solar spectrum extending from A to H. This drawing was much more complete than Angström's; still, as the position of the lines had not been determined very accurately, M. Thollon determined to go over the whole work again, under better conditions, and to make as complete a drawing of the spectrum as possible, which should extend from A to the blue end. The labour, however, was so great that he did not proceed further than the *b* group. A year ago (Sept., 1885,) M. Thollon presented to the Academy a drawing of this portion of the spectrum, containing about 3,200 lines, of which about 2,090 are of solar origin, 866 telluric, and 246 may be regarded as mixed, that is to say, resulting from the superposition of telluric and solar lines. The drawing is divided into four longitudinal bands, of which the first shows the solar spectrum when the sun is 80° from the zenith, and the air contains but little aqueous vapour; the second, when the sun is 60°, and the hygrometric state of the air approaching saturation; the third also when the sun is 60° from the zenith, and the air very dry; while the fourth contains those lines only which are of solar origin. By this method not only can the telluric be easily distinguished from the solar lines, but also a comparison can be instituted of the lines seen in different states of the atmosphere. With respect to the measures, the first thing to be done was to measure the distances of the lines with all possible accuracy, so as to preserve the respective values which they should have in the solar spectrum. The $\frac{1}{30}$ of the distance which separates the lines D^1 and D^2 is the unit adopted and represented in M. Thollon's drawing by 1 millimetre. The thickness of the lines was determined by means of a glass needle of a conical form in the micrometer, which was capable of being placed side by side with the line. The breadth of the lines would thus correspond with that of some part of the needle. Nevertheless, the solar spectrum is not so invariable as one might imagine. M. Thollon gives a reproduction of the part of the spectrum between B and C as drawn by himself and by Angström. The latter shows 29 lines, the former 146. In Smyth's three Winchester spectra the numbers are, respectively, 102, 114, and 95.

On Hitherto unrecognised Wave-lengths. By S. P. Langley. (Reprinted from the *American Journal of Science*, August, 1886.)

In this paper Prof. Langley gives the results of his observations, undertaken as a sequel to those of which an account was published in March, 1884. In the former paper the wave-lengths in the solar spectrum were fixed by direct measurement, as far as about 23,000 of Angström's scale. At this point the solar heat was so feeble that it was only just capable of measurement with the instrument employed in 1881; for, though the spectrum of the infra-red can be studied with comparative ease with a prism, the grating greatly diffuses and weakens it. The later observations were undertaken, not primarily to settle the theoretical questions involved in determining the relation between dispersion and wave-length, but with the object of providing a way which will hereafter enable any observer to determine the visible or invisible wave-lengths of any heat, whether from a celestial or terrestrial source, observed in any prism; and thus to gain that knowledge of the intimate constitution of radiant bodies, which an acquaintance with the vibratory period of their molecules can usually alone afford us. The apparatus used by Prof. Langley is somewhat complicated, and a mere description without an explanatory diagram would be unintelligible. The bolometer employed was of the usual sort; but serious difficulties presented themselves in obtaining a galvanometer of sufficient accuracy. A modification of Sir W. Thomson's reflecting galvanometer was devised, of such extreme delicacy that, in order to obtain corresponding accuracy in gravity determinations, we should need a balance capable of weighing a kilogram, which would at the same time give an unequivocal deflection of ·001 of a milligram in either pan. The gratings, of which there were three, were ruled by Prof. Rowland, and the rock-salt prism was made by Mr. Brashear; with this the Nickel-line between D^1 and D^2 was distinctly shown. Prof. Langley thus sums up the results of his very laborious observations: "Broadly speaking we have learned through the present measures with certainty of wave-lengths greater than 0·005m., and have grounds for estimating that we have recognised radiations whose wave-length exceeds 0·03m., so that, while we have directly measured to nearly eight times the wave-length known to Newton, we have probable indications of wave-lengths far greater, and the gulf between the shortest vibration of sound and the longest known vibration of the ether, is now in some measure bridged over."

An Investigation in Stellar Photography conducted at the Harvard College Observatory. By E. C. Pickering. (Reprinted from the *Memoirs of the American Academy.*) Cambridge, U.S., 1886.

This very valuable paper contains the results of experiments which have been conducted at Harvard College Observatory since 1882. It is very fitting that such observations should be carried on there, for it was at that Observatory that the first photographic image of a star was obtained. On July 17, 1850, a satisfactory photograph of Vega was obtained by Mr. J. A. Whipple on a daguerreotype plate. The experiment was repeated with various stars and clusters, but the work was abandoned, owing to the imperfections of the driving-clock and the lack of sensitiveness of the plates. Both of these defects were partially remedied in 1857, and the work resumed by Prof. G. P. Bond. The results of his researches are contained in three classical papers published in the *Astronomische Nachrichten.* The experiments were afterwards repeated by Messrs. De la Rue and Rutherfurd, followed, after some years, by Dr. Henry Draper, Mr. Common, and others. The first successful photograph of the spectrum of a star, also of Vega, was obtained by Dr. Henry Draper in 1872. The present paper shows the spectra of seven stars, and the author states that the plate containing them appears to be the first engraving of a stellar spectrum obtained entirely by photographic processes. After several preliminary experiments, it was determined, in 1885, to use a Voigtländer object-glass of 8 inches aperture and 45 inches focus, which was partially refigured, so that its focal length should be 14·6 cm. The tube containing the objective was mounted equatorially, and the instrument driven by clockwork. It was therefore possible to obtain either discs or trails on the sensitive plates. From 1882 to 1885 a lens of only 2½ inches aperture had been used. With the larger lens, 117 stars within 1° of the pole, one of them no brighter than the fourteenth magnitude, left trails. The average deviation of the measures of the brightness of these stars on different photographs was less than a tenth of a magnitude, a greater accordance than is given by any other photographic method. A similar result was obtained from the Pleiades, of which group more than 50 stars left trails. Excellent stellar spectra were obtained by placing a large prism in front of the object-glass—a method first suggested and carried out by Fraunhofer, and afterwards by Father Secchi. An exposure of five minutes gives the spectra of all stars brighter than the sixth magnitude in a region 10° square, while with an exposure of one hour the spectra of stars no brighter than the ninth magnitude were obtained. Miscellaneous observations were also secured of the Pleiades, of the nebula in Orion, of Jupiter's satellites, and of various other objects; also of the new star in Orion, and of its spectrum. One plate shows that this star must have been much fainter on Nov. 9, 1885, than at the time of its discovery. The researches of Prof. Pickering are a signal example of what may be accomplished by perseverance and properly directed efforts.

No. 2,746 of the *Astronomische Nachrichten* contains a third catalogue of 100 nebulæ discovered at the Warner Observatory by Lewis Swift. The catalogue comprises the dates of their discovery—ranging from June 18, 1884, to June 8, 1886—their positions and descriptions.

Transit Tables for 1886. Giving the Greenwich mean time of transit of the sun, and of certain stars, for every day of the year; with an ephemeris of the sun, moon, and planets. Computed from the *Nautical Almanac* for popular use. By Latimer Clark, F.R.A.S., M.I.C.E., &c. London: E. and F. N. Spore, 125, Strand, W.C. 1886. pp. 71. Price, 2s. 6d.

We welcome the appearance another year of Mr. Clark's excellent little book. To many it would be of more use than the *Nautical Almanac,* which contains much that they would not require. Appended are description and drawings of Mr. Clark's patent improved transit instruments, with prices and testimonials (A. J. Frost, Westminster Chambers, Victoria Street, S.W.). We wish all success to this praiseworthy attempt to popularize the transit instrument. Clear directions are given for its adjustments and use, and to enable any one to obtain accurate time in any part of the world with a very trifling amount of trouble. A more ready and useful set of monthly tables we cannot imagine; and they are very neatly and clearly printed.

DUN ECHT CIRCULAR, No. 125.

Professor Krueger, of Kiel, transmits a code telegram announcing the discovery of a comet by Finlay (at the Royal Observatory, Cape of Good Hope) on September 26. It is described as circular, 1′ in diameter, not brighter than a star of the 11th magnitude, with some central condensation, but no tail.

It was observed:—

Greenwich M.T.	R.A.	Decl.
h. m. s.	h. m. s.	° ′ ″
Sept. 26 8 0 49	17 2 1·9	S.26 4 6

Daily motion, *plus* 2m. 20s. and 4′ *south.*

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, Sept. 29.

DUN ECHT CIRCULAR, No. 126.

The following elements and ephemeris of comet "Finlay" have been received from Boston, U.S.:—

Elements.

$$\begin{aligned} T &= 1886, \text{ November } 20{\cdot}99 \text{ Greenwich M. T.} \\ \left.\begin{aligned} \pi - \Omega &= 301^\circ\ 25' \\ \Omega &= 51\ \ 26 \\ i &= 3\ \ 19 \end{aligned}\right\} & \text{ Mean Equinox } 1886{\cdot}0 \\ \log q &= 0{\cdot}05865 \end{aligned}$$

Ephemeris for Greenwich Midnight.

1886	R. A.	Decl.	Brightness.
	h. m. s.	° ′	
Oct. 3	17 20 4	S.26 22	1·13
7	17 31 20	26 34	
11	17 42 52	26 40	
15	17 55 36	26 43	1·39

The orbit was computed by Mr. Loss [? Prof. Boss] from observations on September 26, 29, and October 1. It indicates a probable identity with comet 1884 I.

Lord Crawford's Observatory,
Dun Echt: 1886, October 5.

DUN ECHT CIRCULAR, No. 127.

Professor Krueger transmits the following "Scientific Observer" code telegram from Kiel:—"Unharmed Hartwig October angler domineer browbeat [zero] being Krueger"; announcing that a comet was discovered by Hartwig, October 5, 16h. 2m. Greenwich M.T., in Right Ascension 10h. 37m. 24s., and North Declination 1° 3′.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1886, October 7.

DUN ECHT CIRCULAR, No. 128.

In the *Science Observer International Circular* No. 18 (Berlin, Oct. 12) Mr. J. Ritchie, jun., of Boston, U.S., communicates the subjoined elements and ephemeris of the comet discovered by Mr. Barnard on Oct. 4, and by Dr. Hartwig on the following day.

The orbit was deduced by Professor L. Boss, from observations made on October 6, 8, and 10.

Elements.

$$\begin{array}{rcl} T & = & \text{1886, December 11·40, Greenwich M. T.} \\ \pi - \Omega & = & \left.\begin{array}{l} 89^\circ\ 26' \\ 135\ \ 39 \\ 106\ \ 15 \end{array}\right\} \text{Mean Equinox 1886·0} \\ \log q & = & 9·7763 \end{array}$$

(Lines: $\pi - \Omega$ = 89° 26′; Ω = 135 39; i = 106 15.)

Ephemeris for Greenwich Midnight.

1886	R. A. h.	m.	Decl. °	′	Brightness.
Oct. 12	10	52·0	N. 2	5	1·75
16	11	1·6	2	47	
20	11	12·0	3	34	
24	11	23·5	4	28	
28	11	36·5	5	29	
Nov. 1	11	51·5	6	41	
5	12	8·7	N. 8	0	5·59

Brightness on October 4 taken as unity.

To give a rough idea of the path of the comet, Dr. L. Becker has computed the following extension of the ephemeris.

1886-7	R. A. h.	m.	Decl. °	′	Brightness.
Dec. 3	16	21	N. 15	18	29·2
19	18	49	4	21	17·2
Jan. 12	20	24	S. 9	14	3·6
Feb. 12	21	28	19	44	0·9

Lord Crawford's Observatory,
Dun Echt: 1886, October 16.

THE FIVE INNER SATELLITES OF SATURN.

From a discussion of the observations of these satellites, made at Toulouse in 1876 and 1877, Prof. Asaph Hall has arrived at the conclusion that they can be satisfied within the limits of their probable errors by *circular* orbits. It was hoped that the Washington observations would determine the position of the lines of apsides with such accuracy that the motions of these lines would become known, and that data might thus be obtained for a new determination of the mass of the Ring, and of the figure of the planet. If the motion of these lines could be determined, a comparison of the elements of the inner satellites with those of Titan would enable us to separate the action of the Ring from that of the figure of the planet. But the resulting circular orbits for the inner satellites make the position of a line of apsides indeterminate, and for the present the mass of the Ring remains unknown. Prof. Hall communicates to the *Astronomische Nachrichten* the result of a comparison of the observations made at Toulouse with his elements. It will be found in No. 2743 of that Journal.

SATURN'S RING.

Nov. 21. Major axis of ring = 44″·58
Minor „ = 17″·23
Inclination of earth to plane of ring = 22° 43′·8 S.
Mean obliquity of the ecliptic, Nov. 16 = 23° 27′ 14″·27
Apparent „ „ „ „ = 23 27 5 ·83
Semidiameter of the sun = 16 13 ·1
Parallax of the sun = 8 ·95

Sirius, Sept., 1886.

MINOR PLANET 252.

M. Charlois has communicated to the *Bulletin Astronomique* the following elements of this planet:—

Epoch, 1885, Dec. 4·5, M.T. Paris.
M = 31° 10′ 4″·98
π = 355 8 5 ·00 }
☊ = 203 18 39 ·43 } Mean Equinox, 1885·0
i = 10 1 36 ·64 }
ϕ = 4 47 56 ·53
log a = 0·4990238
log μ = 2·8014709
μ = 633″·0979

ELEMENTS OF COMET b, 1886 *(BROOKS).*

From observations at the Dudley Observatory on May 1, 5, 9, Mr. Lewis Boss has computed the following elements:—

T = 1886, May 4·952. G. M. T.
☊ = 288° 31′·1 }
ω = 39 37 ·4 } App. Equinox.
i = 100 50 ·1 }
loq. q = 9·2588

Science Observer.

MINOR PLANET 259.

Professor C. H. F. Peters announces the discovery of another Planet, 259. Its place on June 28·7591 G. M. T. was R.A. 17h. 28m. 50s., decl. 13° 7′ S. Motion slow S.

WINNECKE'S COMET (1819 *PONS*).

This comet was discovered at Cape Town on August 20·2416 G. M. T. in R.A. 13h. 10m. 21·5s., decl. 1° 8′ 17″ S. The comet is described as of about mag. 10, circular, without tail, nebulosity 1′ in diameter, with some central condensation. *Science Observer.*

THE PLANETS FOR NOVEMBER.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	15 44 21	S.22 5	5″·4	1 1·4
	9th	16 28 9	S.24 27	6″·0	1 13·6
	17th	17 3 5	S.25 17½	7″·2	1 17·0
	25th	17 12 31	S.24 12½	8″·8	0 54·8
Venus ...	1st	14 2 21	S.11 12	9″·8	23 15·7
	9th	14 41 4	S.14 40	9″·8	23 22·9
	17th	15 20 59	S.17 44½	9″·8	23 31·3
	25th	16 2 13	S.20 18½	9″·8	23 40·9
Saturn ...	1st	7 37 6	N.21 17½	17″·2	16 51·6
	9th	7 36 58	N.21 18½	17″·4	16 19·9
	17th	7 36 19	N.21 20½	17″·6	15 47·9
	25th	7 35 12	N.21 23½	17″·8	15 15·3
Neptune ...	1st	3 40 4	N.17 45	...	12 55·2
	17th	3 38 16	N.17 39	...	11 50·5

Mercury sets half an hour after the sun on the 1st, the interval increasing.

Venus rises about half an hour before the sun, at the beginning of the month, the interval decreasing.

Saturn rises at 8h. 49m. p.m. on the 1st, and afterwards earlier each night.

Books Received.—Results of Meteorological and Magnetical Observations at Stonyhurst College, 1885. By the Rev. S. J. Perry. Manresa Press, Roehampton.—Sirius. Leipsic.—Astronomische Nachrichten.—Cape Meridian Observations, 1879 to 1881, under the direction of David Gill, LL.D., F.R.S.—Greenwich Spectroscopic and Photographic Results, 1884.—Greenwich Astronomical Results, 1884.—Introduction to Greenwich Astronomical Observations, 1884.

ASTRONOMICAL OCCURRENCES FOR NOVEMBER, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites		Meridian Passage.
		h. m.			h. m. s.	h. m. α Persii.
Mon	1	5 15	Near approach of *d* Sagittarii (5) Sidereal Time at Mean Noon 14h. 42m. 47·15s.			12 31·9
Tues	2		Sun's Meridian Passage 16m. 18·34s. before Mean Noon			12 28·0
Wed	3	5 5 3 16 4 32	☽ Moon's First Quarter Occultation of B.A.C. 7263 (6) Reappearance of ditto			12 24·1
Thur	4					12 20·2
Fri	5					12 16·2
Sat	6					12 12·3
Sun	7	5 45 6 32 6 0 7 1 6 23 7 42	Occultation of 4 Ceti (6) Reappearance of ditto Occultation of 5 Ceti (6) Reappearance of ditto Occultation of B.A.C. 5 (6) Reappearance of ditto			12 8·4
Mon	8			1st Sh. I. 1st Tr. I. 2nd Ec. D.	17 2 17 32 18 19 33	12 4·4
Tues	9	6 4 7 9	Occultation of ν Piscium (4½) Reappearance of ditto	1st Oc. R.	17 3	12 0·5
Wed	10			2nd Tr. E.	16 45	11 56·6
Thur	11	7 6	○ Full Moon			11 52·6
Fri	12	7 18 8 18 9 17 10 25 14 38 15 37 14 46 15 57 15 6 15 39 16 46 18 27 19 16	Occultation of 48 Tauri (6) Reappearance of ditto Occultation of γ Tauri (4) Reappearance of ditto Occultation of 75 Tauri (6) Reappearance of ditto Occultation of θ^1 Tauri (4½) Reappearance of ditto Near approach of θ^2 Tauri (4½) Occultation of B.A.C. 1391 (5) Reappearance of ditto Occultation of α Tauri (1) Reappearance of ditto			11 48·7

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Persii. —
Sat	13	16 2 17 11	Occultation of 115 Tauri (6) Reappearance of ditto			11 44·7
Sun	14		Sidereal Time at Mean Noon 15h. 34m. 2·36s.			11 40·8
Mon	15		Illuminated portion of disc of Venus=0·993 Illuminated portion of disc of Mars=0·954	**1st** Sh. I.	18 56	11 36·9
Tues	16	1	Conjunction of Moon and Saturn 3° 3′ N.	**1st** Oc. R.	19 3	11 32·9
Wed	17		Sun's Meridian Passage 14m. 52·07s. before Mean Noon	**2nd** Tr. I **2nd** Sh. E.	16 52 18 18	11 29·0
Thur	18	10 40 7	☾ Moon's Last Quarter Opposition of Neptune to the Sun			11 25·1
Fri	19		Saturn's Ring: Major axis=44″·44 Minor axis=17″·16	**3rd** Sh. E. **3rd** Tr. I.	17 40 17 58	11 21·2
Sat	20					11 17·2
Sun	21	16 42 17 31 16 58 18 36 19 8	Occultation of 46 Virginis (6) Reappearance of ditto Near approach of *k* Virginis (6) Occultation of 48 Virginis (6) Reappearance of ditto			11 13·3
Mon	22	16 42 17 40 13	Occultation of B.A.C. 4647 (6) Reappearance of ditto Conjunction of Moon and Jupiter 3° 0′ S.			11 9·4
Tues	23			**1st** Ec. D.	18 7 54	11 5·4
Wed	24			**1st** Sh. E. **2nd** Sh. I. **1st** Tr. E.	17 31 18 13 18 15	11 1·5
Thur	25	7 18 2	● New Moon Conjunction of Moon and Venus 4° 41′ S.			10 57·5
Fri	26	9	Conjunction of Moon and Mercury 5° 41′ S.	**2nd** Oc. R. **3rd** Sh. I.	16 49 19 15	10 53·6
Sat	27					10 49·7
Sun	28	2	Conjunction of Moon and Mars 5° 8′ S.			10 45·8
Mon	29					10 41·8
Tues	30					10 37·9

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	° ′		° ′		° ′
Nov. 1	+24 36M.	Nov. 11	+82 54 E.	Nov. 21	−38 47 E.
2	12 27	12	70 43	22	50 56
3	0 16	13	58 34	23	63 7
4	−11 54	14	46 23	24	75 16
5	24 5	15	34 12	25	87 26
6	36 15	16	22 3	26	+80 23M.
7	48 25	17	9 52	27	68 14
8	60 36	18	− 2 17	28	56 4
9	72 46	19	14 27	29	43 55
10	84 57	20	26 37	30	31 45

Moon farthest from Earth, Nov. 5, 6h.
„ nearest to „ „ 20, 19h.

ASTRONOMIAL REGISTER—Subscriptions received by the Editor.

To Dec., 1885.

Shawcross, W.

To Jan., 1886.

Johnson, R. C.

To June, 1886.

Campbell, Lieut.-Col.

To Sept., 1886.

Cooke & Sons, Messrs.

To Dec., 1886.

Hemming, Rev. B. F.
Herschel, Col.
Perigal, H
Tebbutt, J. E.
Waldegrave, Hon. H. N.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, Chief Office, London.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 288. DECEMBER. 1886.

ROYAL ASTRONOMICAL SOCIETY.

Session 1886-87.

The first meeting after the vacation was held at the Society's apartments, Burlington House, on Friday, November the 12th, 1886.

J. W. L. Glaisher, Esq., *President,* in the chair.

Maurice Loewy, of the Observatory, Paris; and G. Spörer, of the Astrophysical Observatory, Potsdam, were, on the recommendation of the Council, elected Associates of the Society.

Col. Tupman announced that 168 presents had been received since the last meeting. Amongst them were some very remarkable copies of photographs taken by M. Gothard.

Mr. Knobel: I should like to call attention to some of these photographs, or rather paper prints from photographs, received from M. Gothard. One of them is particularly deserving of notice. It represents the ring nebula of Lyra, and is very remarkable as showing the inequality in the brightness of the ring. It is quite in accordance with the drawing made of the nebula at Washington. These photographs have been made with an exposure of from 50 to 70 minutes, and they show a great number of very minute stars; some of which I have been able to identify. In one photograph of the Horseshoe nebula there are all the stars, but there is not the slightest trace of the nebula, whereas in the Andromeda nebula, the nebulous light is photographed very strongly, and the Dumb-bell nebula is slightly indicated. The Horseshoe is a pretty bright nebula in a telescope; and it is very remarkable that there should not be the slightest trace of the nebulous matter in the photograph which M. Gothard has made.

Mr. Downing: With regard to the Lyra photograph of the ring nebula, the Brothers Henry of Paris, commenting on it, say they have taken photographs of the same nebula, and they found a perfect circle with light extending all round. This is elliptical.

A vote of thanks to the donors of the presents was then passed.

Father Perry described a paper he had written *On the bands in the spectra of sun spots observed at Stonyhurst.* He said: A good many of you are aware that during the last three or four years, we have been engaged at Stonyhurst in examining the spectra of sun spots at the red end. A great deal has been done between D and the violet end, and a great deal of splendid work has been done at South Kensington by Mr. Lockyer and others. Seeing that no person was working at the red end, Mr. Cortis and I got to work at it. I have given in the paper some few results he has obtained, and there are a number of others which I hope to give in future papers. There are three things I should call attention to. There are two sorts of absorption, general absorption, and selective absorption. But the general absorption, to use a bull, is selective, that is to say it is local on certain occasions. With regard to selective absorption, the lines are sometimes darkened, and sometimes widened. But there is another phenomena I call attention to in the paper, and that is the penumbra of the lines. Sometimes the lines are widened, but the dark line is not widened, there is merely a shadow or wing on either side; such a shadow is very frequently seen near the D lines, and with two very fine sodium lines on the red side of D. But I want to call particular attention to the special bands that are observed. We have observed eleven at Stonyhurst, and have kept our attention especially fixed on two—6380, and 6383. Immediately you pass away from the spots you always lose the bands, not only in this but in every case. You get complicated spectrum bands only in the spot spectra. With regard to both the lines, 6380, and 6383, they have been on one or two occasions split up into about three lines; and in some cases when we have found bands in the spot spectra, in following these out into the photosphere we have generally lost them entirely, but sometimes there are fine lines in the photosphere. In regard to all these bands there is no single one, with the exception of 6380, that has any corresponding lines in the ordinary solar spectrum: not one! 6383 we had known in Angström, but we find in the two spectra published lately by Professor Smyth, and by Fiévez, two lines in the ordinary spectrum, and Smyth has three; but with those two exceptions, all the other bands have nothing corresponding to them in the ordinary solar spectrum. Another thing is that nearly all of them correspond with the position of bright lines in the chromosphere as observed by Professor Young. The points I wish to mention are: 1st, that the bands observed in sun spots have no lines corresponding to them in the ordinary photospheric spectrum, and that the bands have bright chromospheric lines corresponding to them.

Mr. Ranyard: With regard to the observations of bands in the spectrum of solar spots, one is liable to be very much deceived by the mere alteration of illumination in the spectrum. My faith in what I had thought previously to be veritable broadening of lines and bands in sun-spot spectra has been very much shaken by an experiment recently shown me by M. Thollon. While I was looking at the solar spectrum he placed a small object at a little distance in front of the slit, so as to throw a shadow on the slit, a shadow which was almost total at the centre, but had a penumbra on either side. It gave rise to a band across the spectrum which I immediately took to be the spectrum of an ordinary sun-spot. In the dark band the lines appeared winged and thickened, and quite different from their appearance in the photosphere spectrum. I have recently repeated the experiment near to a sun-spot, and I would advise others to do so. It has quite shaken my faith in the objective character of many of these winged lines and bands. I do not wish to suggest that there is no change in passing from the ordinary solar spectrum to the sun-spot spectrum, but a change of illumination; but the evidence with regard to any particular line or change should be carefully examined in conjunction with the solar spectrum seen under fainter illumination.

Mr. Maunder: I have heard Father Perry's remarks with very great interest. Some years ago, in observing the spectra of sun spots, I found a number of shaded bands in the region of the *b* lines. I did not examine the red region in the spectrum carefully for bands, and I was not able to find bands similar to those near *b* anywhere else except towards F; but I do not think there can be any mistake about the reality of the bands which I saw. They were altogether different, it seems to me, to any effect that could be produced by such a darkening of the solar spectrum as M. Ranyard has described. They were of considerable breadth, and were very distinct, and in their main characteristics they resembled very much those which Father Perry has described—that is to say, for the most part we were unable to see any lines in the solar spectrum corresponding to them, although occasionally we would be able to trace the band to a very fine faint line in the solar spectrum in the neighbourhood of the spot; I believe none of the bands were given in Angström, but one or two were represented in Fiévez's chart; and on one or two occasions I was just able to break up a band into two or three fine lines, so that these bands in the green presented exactly the same characteristics as those which Father Perry has been able to find in the red end of the spectrum.*

Votes of thanks were passed to Mr. Cortie and to Father Perry for the paper.

Mr. Isaac Roberts then described his paper on *Star Photography.* He said: The negatives which I now exhibit were taken with my 20-inch reflector on the nights of the 17th, 23rd, 24th, and 25th August this year, and they represent parts of the constellation Cygnus, corresponding with photographs by the Brothers Henry, taken with their 13-inch refractor in June and August, 1885, copies of which have been presented to the Society, as well as to some of the Fellows. The enlargements also now exhibited have been made to a scale corresponding to that adopted by the Brothers Henry so as to facilitate comparisons between them. The time of exposure, namely 60 minutes, is common to all the photographs; though there are two in duplicate, one of 34 minutes, and the other of 60 minutes, of the same area. If we compare these plates with those by the Brothers Henry of the same area, there are striking differences perceptible in the sharpness of the margins of the star discs, in the density of the images, and in the number of stars photographed in 60 minutes by the two instruments respectively. In the Henry plates the margins of the stars are remarkably sharp and clearly defined, and the brightness or the density of the images, whether the stars are of the 2nd or 15th magnitude, is equal, or nearly equal, in all without any perceptible gradation. The reflector, on the contrary, shows the margin somewhat undefined, and stars of the 1st to the 3rd magnitude show prominently defraction rays round them. Not less noticeable is the gradation of the light of the stars of different magnitudes. The gradation is such that the stars are ultimately lost in the colour of the background, or film, and to define their magnitudes will severely tax the powers of classification. In drawing inferences from the relative numbers of the stars photographed by the two instruments in 60 minutes, it must be remembered that besides the difference between them in focus and aperture, as well as achromatism, there may be a difference in the sensitiveness of the plates used, and also a difference in the atmospheric conditions when the photographs were taken, as well as a difference in the method of developing the images; but, subject to these reservations, we may, in the absence of more exact methods, make an approximate comparison by counting the number of stars on a given area, such as a square inch, on the enlarged photographs respectively. In this manner I counted on one of the reflector photographs 109, 93, and 70, or an average of 91 stars on one square inch. The numbers on the corresponding area of the refractor photographs were 59, 64, and 41, or an average of 55 stars on one square inch. It therefore appears that the reflector has over the refractor the advantage in the number of stars photographed in the average ratio of nearly two stars to one. In view of others entering upon the work of photographing stars, and to save them much waste of time and resources, it would be desirable to make comparisons between the relative efficiency of existing refractors and reflectors as instruments for photographing. Comparisons may be made as follows: First, a sufficient number of photographic dry-plates should be obtained from some reliable maker. These should be made from one uniform mixture of emulsion, and the plates distributed amongst those who would engage in the inquiry. Secondly, a given number, such as ten, of the plates should be exposed for 30 minutes each on ten clear nights—one plate each night—upon such sky spaces as may be determined on. Thirdly, the plates should be developed by the same chemical formula, with an immersion in the developer of 5 minutes. Fourthly, the number of stars on each plate should be counted, and the density of the images noted. The result would be some guide to those who intend to engage in photographing stars, and enable them to choose their instruments, and to compare their work afterwards. In addition to the information thus to be obtained a report upon the driving clock used in the experiment should be prepared, and its errors given during each exposure of the plates. An accurate and reliable driving clock and gearing are absolutely necessary in star photography, as any appreciable error would distort the star discs, and so prevent accurate micrometrical measurements of their relative positions being made.

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* I should like to add to the above remarks that the true objective character of the bands near *b* is proved by the following facts:—They form a regular definite series, occupying the same positions in different spots. They are not seen in all spots, and may be invisible in the darker of two spots, and visible in the fainter. They vary—so far as present observation goes—with the spot-cycle, and become less noticeable after the maximum is passed.—E. W. M.

Mr. Roberts then described another paper on some negatives taken by him of the nebula in the Pleiades. He said: These are original negatives of the nebula of the Pleiades, and I have also, a tracing of the Chart, which the Brothers Henry have published in the Annals of the Paris Observatory, so that Fellows who may not have had an opportunity of seeing the Chart may understand the advance which has been made. On the night of the 23rd October last, I had an opportunity of taking a photograph of the Pleiades, and intended to expose the plate for three hours so as to be able to compare the result with that obtained by the Brothers Henry; but the clouds caused me to stop the exposure after 89 minutes. The plate when developed showed clearly that the stars Alcyone, Maia, Electra, and Merope are all surrounded by nebulous haze, which is traceable only about three of the stars on the Chart of the Brothers Henry, as shown in the Annals of the Paris Observatory. On the following night, the 24th, I exposed another plate for three hours, which, after development, showed that not only are the stars which I have just named surrounded by nebulæ, but that the nebulosity extends in streamers and in fleecy masses till it seems almost to fill the spaces between the stars and to extend far beyond them. It suggested the probability that these principal stars of the Pleiades, together with many of the stars around them, are enveloped in one vast nebula. The negatives and the enlargements to six diameters, which I now exhibit, will enable you to form your own judgment as to the credibility of the evidence upon which these inferences rest; and I await with watchfulness a clear interval, which will enable me to try an exposure of five hours, in order to obtain more light upon the subject. The star discs upon the photograph are somewhat deformed by defraction and incorrect instrumental movement, but I think, you will not have much difficulty in mentally making the corrections necessary to form an accurate judgment; and I anticipate being able to obviate this slight distortion in future exposures.

Mr. Common: There are one or two remarks I should like to make. I should like to say, touching these negatives Mr. Roberts has handed round, that I am extremely gratified to see what Mr. Roberts has done, not only because it has been done by a person in England, but because it has been done with a reflector. I have always had a good deal of faith in reflectors, not because I happen to have one. In the early days of 1876, when I took up photography, I came to the conclusion that if you were to do any good with photography you must use a reflector; and that is more than borne out by the great changes that have taken place in photographic processes, especially since the use of bromide plates, which are sensible to a much larger portion of the spectrum than the old collodion plates. If you want the most effective action of the light, you must have a telescope which will collect it all to a point. You cannot by any means make a refractor work perfectly. It is at best a compromise. The dispersion of the different wave lengths with the present qualities of glass can never be obviated. There are always cones of rays outside the focus selected, and they will have their effect on the plate; whereas in the reflector you bring all the rays to the point where you want them, without trouble, to say nothing of the fact that in working with the reflector you can get, for the same amount of money or labour, a larger aperture, and consequently more light than with a refractor. The reflector, we know, has its disadvantages, and they are such as give a good deal of trouble, particularly owing to the tremors of the reflector, which causes a great deal of displacement of the image, which does not happen with the refractor. There is another objection to the reflector, that the tubes must necessarily be open for photographing, and that allows air currents to be formed, which the refractor does not suffer from. But the images which the reflector gives are smaller. Mr. Roberts stated, that he has compared the enlargements he has made with those of the Brothers Henry: the paper prints of the Brothers Henry are purely black and white, whereas his own show half tints. If he wishes to point out any differences between the refractor and reflector, on that score I join issue with him at once. I explained some time ago that half tints can be produced by proper manipulation, so that you can bring a negative showing the gradations of light to an absolutely black and white photograph. I explained before, that to make any fair comparisons between the action of the reflector and refractor you must not deal with the paper prints, or the enlargements, but with the original negatives. I have been favoured by the Brothers Henry with several of their transparencies from the original negatives, and being direct copies, there was not that chance of diminishing the half tints; and I found on comparison with my own photograph of the nebula of Orion, which is a fair example of a reflector's work, there is less half tone in the reflector's work than in the refractor's. There is one other point, and that is, as to the images of the stars themselves. In ascertaining the length of exposure required to photograph the different magnitudes of stars, the Brothers Henry found that a light ratio of 2.5 held closely, and so it does; and that fact gives rise to some interesting points—first as to how that wonderful aggregation of the silver takes place, for we have the remarkable fact, that a little point of light falling on a plate will so agitate the molecules of silver as to carry the photographic action a long way from that centre—in some cases so far as to produce solarisation and to get a reversal of the image in the centre.

Mr. Ranyard: I would like to draw the attention of the meeting to the very beautiful photographic images, which Mr. Roberts has obtained of the diffraction rays, produced by the supports of the photographic plates; they are to be seen round the images of all the larger stars: down to the third and fourth magnitude they can be perceived. Though they make the whole star appear larger, the central point which would be used for measurement, is very much smaller than the central disc on the Henry plates; and there are certainly a greater number of stars to be counted on Mr. Roberts' plates than on those of the Brothers Henry. I can only congratulate him very much indeed upon his success. There are eight rays around the larger stars. On enquiry of Mr. Roberts I find that besides the three watch-spring supports which give rise to the six rays usually seen in reflectors, there was a focusing rod, which gives rise to the two other rays.

Father Perry: I ought to say a word or two about these photographs, as I am probably the only person in the room who has had the pleasure of visiting Mr. Roberts' Observatory, and examining the plates with the microscope, and comparing them with the Brothers Henry's work, and the negatives with the prints. I only wish, with years and years of labour, I could produce anything like the results Mr. Roberts has obtained. There are defects in the plates, and I think there is no person more willing to acknowledge this than Mr. Roberts himself. As to Mr. Common's remarks about expense, there will be no difficulty here. We have unlimited generosity, and if anything can be produced for money it will be produced by Mr. Roberts. The chief difficulty at present is owing to the driving clock not being perfect; but I am happy to say that Mr. Roberts is likely to remedy that. Mr. Grubb feels perfectly certain that he will be able to produce a clock which will not require any eye-watching at all. That will be an immense advantage. As to the photograph of the Pleiades nebulæ I do not know whether Mr. Roberts brought out this point, but it is necessary to be brought out, that his photographs have been compared with the photographs obtained by the Brothers Henry, and every mark in the photographs of the Brothers Henry is in the photographs of Mr. Roberts, and along with that ten times as much; and after an exposure of five hours there is little doubt that he will get a great deal more nebulous light in the cluster of the Pleiades; whether we shall get the whole of it in the field I do not know. There is no doubt Mr. Roberts has added six or seven nebulæ already in that part of the heavens which have not been suspected to exist before. He is undertaking to photograph the whole of the heavens from the North Pole down to 10 degrees below the equator, and he hopes to be able to do that unassisted. I think he will have to get an assistant to help him, but so far he has done the entire work himself.

Mr. Common: On Mr. Roberts' second paper in regard to the negatives of the Pleiades, I would say a few words. I suppose the comparisons made by Mr. Roberts with the plates of the Brothers Henry were in both cases derived from the paper prints, and not from the negatives? [Mr. Roberts: Yes.] Not that the comparisons would not be perfectly fair, because the relative apertures are as 312 to 130, and of course aperture is of the very first importance in the matter of showing faint stars. Not only is the aperture that Mr. Roberts used twice the area of that used by the Brothers Henry, but the focal length is as 100 to 132. Of course, theoretically, with stars that should make no difference, but in nebulous objects of a certain diameter it makes a great deal of difference, and the advantage is very much in favour of the reflector. That the amount of nebulous matter shown is greater than that in the Henry photographs is due entirely, I think, to the aperture used. We have never before seen a picture of the Merope nebula extending half a degree away from the star in the direction where it has generally been shown by those observers who have seen it with the naked eye.

Mr. Roberts: The paper deals with the question Mr. Common raises, and when it comes to be read, if it should be printed, it will be seen, I think, that I guarded myself against claiming for the reflector anything more than all who examine the photographs will be prepared to grant. The comparisons have been made with the enlarged photographs in both cases, and they have been made with care, and studiously without bias to favour the one more than the other. If I saw a doubtful star on my own plates I rejected it in every case. But Mr. Common did not quite understand the point I wished to raise with regard to gradation. In the Henry plates the stars, from the smallest to the largest, if examined with a magnifier, appear equally bright. Of course, they are not of the same diameter, but the same intensity of light appears to prevail both in the 15th magnitude, and in the brightest, which may be of the 3rd or 4th magnitude. What I mean by the gradation in the reflector photographs is that the light diminishes imperceptibly from the brightest star to the faintest, not in a certain part of the disc, but the whole of the disc graduates. Therefore, there is a striking difference between the performance of the refractor and the reflector: that the one shows stars as we see them in looking through a telescope, and the other shows them without the gradation we see there.

The President asked Mr. Maunder if he had any remarks to make on the recent eclipse.

Mr. Maunder: I did not come prepared to give any account of our doings in the West Indies. My own part in the matter was a comparatively small one. I may say that we met both on the Island of Grenada and at Carriacou with the greatest hospitality and the most thorough assistance from the residents, and also from the officers of the two gunboats who were appointed to carry us to our destinations. We divided into several parties when we arrived at Grenada, in order that if the weather was unpropitious in one part the entire expedition should not be a failure, and as it proved, the precaution was not at all unnecessary. The South of Grenada was occupied by Capt. Darwin, Dr. Schuster, and Professor Thorpe; Mr. Turner and Professor Tacchini went to the east coast of Grenada; Mr. Lockyer to Green Island, off the north-east point; and Father Perry and I went twenty miles further north, to the Island of Carriacou. On the morning of the eclipse it was very fine up to sunrise. The sun rose in a fine sky, but almost immediately afterwards heavy banks of clouds came up, and before the first contact the sun was hidden, and we had a smart shower a little before totality, which obliged us to cover up our instruments; but fortunately before the sun was entirely hidden the clouds cleared away, and we had a very good view of the eclipse in a clear portion of the heavens. About two minutes after totality was over the clouds began to gather again, and a second shower came on very shortly after. At Green Island Mr. Lockyer was unfortunately unable to see anything. The clouds which had so nearly spoiled our view entirely concealed the sun from him. The observers on the east coast of Grenada, and on the south, had fine weather and a clear sky during part of totality. They each, however, lost a certain number of seconds. Professor Thorpe made a very successful series of observations of the intensity of the coronal light. Captain Darwin obtained a number of photographs of the corona. He photographed the sun both before totality and during totality, his object being to test Dr. Huggins' method of photographing the corona in full sunshine; and so far as it appears, his experiment does not support the idea that Dr. Huggins has been successful. Dr. Schuster got two photographs of the spectrum—one with the slit radial and one with the slit tangential. I think both will be very useful—one certainly will; and he secured five photographs of the corona, of which three were very good. I got seven photographs of the corona, and I think five are good;

I was not successful with the photographs of the spectrum. Father Perry and Mr. Turner observed with the spectroscope. Mr. Turner was able in some degree to confirm the observations which Mr. Lockyer made in Egypt in 1882. Professor Tacchini made some very interesting spectroscopic observations. Perhaps the one of the greatest interest was that he showed that the prominences that we see with the eye during a total eclipse are not precisely the prominences which we see by the spectroscopic method in full sunshine; that the prominences seen in the eclipse are larger than those seen by the spectroscope; and that the prominences in their upper portions give a continuous spectrum. I think those are the principal points that we have as yet gathered. There is one little point Mr. Knobel has reminded me of—I believe it is purely physiological. Although I was looking at the corona with the eye all the while I was taking photographs, and I had a good view of the eclipse, yet I never saw any red flames. I saw one or two prominences, and they appeared of a bright silver-white, and I have identified them on the photographs since. There is no doubt I saw real prominences, but they did not produce the slightest red effect on my eyes. I should have said the prominences were a pure white. I saw no trace of colour in the chromosphere or prominences. I suppose that was due to some physiological defect in my eye at the time, but I think it worth while to mention it.

Mr. Brett: It is no physiological defect at all. They are mostly white; they are only occasionally red.

Mr. Knott: Were there not some observations of Prof. Tacchini on the flash?

Mr. Maunder: Yes; but I fear I cannot help Mr. Knott in that respect, as I have not been able to learn exactly what it was that Prof. Tacchini actually saw. I gather that it was, that he found that what has been called Young's reversing layer lay at a much greater height than had been supposed before. The spectrum of bright lines seen at the moment of totality was generally believed to be due to the very lowest layer in the sun's atmosphere; but Prof. Tacchini seems to have traced it to a greater height from the sun.

Father Perry: With regard to the red prominences, I do not think I ever saw such a beautiful sight as the projection of the corona upon the slit of my spectroscope. Unfortunately, I had an assistant at the spectroscope. If I ever go on another expedition, I shall do it all myself, except the registering, because you are more satisfied with what you are doing than if anybody else does it. I had one of Alvan Clarke's refractors. It was grandfathered by Mr. Dawes, and afterwards all Mr. Webb's work was done with that objective. I had the pleasure of using it during the eclipse. The picture through that slit was something magnificent. The red prominences were small, not large; and I think that if any one had tried to see the red prominences by the naked eye, he might have missed them. But everything was so sharp that you could see all the details of the corona, and I only wish I could have spent my time in making drawings of them; but my work was to find carbon, if I could, to confirm the observations made by Professor Tacchini, in 1883. He went to the Caroline Islands, and he was supposed to have found carbon. It was very important to verify that observation, whether it was true or false. Professor Tacchini went out himself to try and verify his observations; and I was told by the committee of the Royal Society to pay attention to that, and that only, during totality. Consequently, I cannot speak to other things, as far as I am concerned, because I had an assistant. I looked in vain for carbon in the spectrum of the corona, and Professor Tacchini did the same at the station at which he was placed, but he saw no carbon either. That is negative evidence rather strong; but, of course, in negative evidence you never have anything so satisfactory as in positive evidence; but all it comes to is, that we could not detect carbon in 1886. Possibly Professor Tacchini did see carbon in 1883. I do not know, but it may have been detected by Dr. Schuster in 1882, and by Mr. Lockyer earlier still. I started in the *b* region, as you know one of the carbon bands nearly touches the *b* lines, and then extends off towards the violet, and there is another carbon line towards the red end. I had just enough of the spectrum in view to keep these two strongest lines of the carbon spectrum in the field of view, and I saw nothing of them; so, with regard to my principal work during totality, I saw nothing. Mr. Turner and I were the only two who were not working at photographs. With regard to the photographic work, we must wait until the photographs are considered in comparison with the photographs taken before. We had magnificent photographs taken in 1871 and 1882, but of those taken in 1883 I do not know very much.

Mr. Stone: May I ask if there was any great extension of the outer corona seen in the last eclipse? Whether there was any relationship between the plane of the ecliptic or the sun's equator, and the extension of the outer corona?

Father Perry: With regard to that, some were rather expecting that we would have a corona with rays coming out from the poles, and a great extension in the direction of the sun's equator. But we did not get that at all.

Mr. Stone: No one saw it?

Father Perry: No one saw a picture of that sort. Captain Masterman, captain of the *Bullfrog*, who is an exceedingly good artist, and generally spends his spare time on shore in making water-colour sketches, saw more or less the same sort of picture that they got near Manilla in 1868.

Lieut.-General Tennant described a paper he presented on the orbit of Comet II., 1883. He said: Last year Mr. Bryant gave us a paper on the orbit of Comet II., 1883, which was observed in the southern hemisphere in the beginning of 1884, and he deduced for the orbit an ellipse of ninety years' period, which has been accepted apparently by the Bureau des Longitudes. I had some doubts with respect to this orbit, and I was induced to compute the orbit again from normal places, obtained from a comparison of the whole of the observed places got in the southern hemisphere. It is true I found that when you rigorously complied with the normal places, the most satisfactory result was an ellipse, but it is an ellipse of something like two thousand years' period, which is a very different thing from what Mr. Bryant found; but as a matter of fact, the doubt as to even the normal places was so great that I think the most probable result is, that the real orbit is not sufficiently correct to differ sensibly from a parabola. I have found the probable error of the places as five seconds, but, looking at the general run of the observations, I do not think that is an improbable error. Of course, if you take three normal places, and rigorously determine the elements, that will simplify the work; you will get an ellipse, or a hyperbola, or, possibly, a parabola, which will correspond to those conditions; but when you come to consider the probable errors critically, you see that the observations are all fairly good ones; but that certain allowance must be made for the object not being well defined, and for the centre of light of the comet probably not corresponding with its centre of gravity.

Mr. Bryant: How many normal places have you taken?

Lieut.-General Tennant: Three normal places, but two of them were indifferent. I do not think you can get better normals. The result will depend on the probable errors you assign to the places; and I think, on the whole, the parabola will give the best result, with a probable error of 5 seconds.

Mr. Bryant: I spent a considerable time over the orbit of this comet, and I could not find a parabola to satisfy the observations in a proper manner, although I could force an agreement. I was rather allured into an ellipse, because I spent so much leisure time on the parabola, that I hardly thought it worth while to leave the subject in that state. I had previously made comparisons with all the published observations of the comet; but I found no material whatever from which I could get normal places. The observations at Melbourne were very discordant; those at Madras were little better; two observations by Mr. Biggs, at Launceston, in Tasmania, had errors too large to be considered; and the only reliable observations were Mr. Tebbutt's. Still I did not care to reject the other observations, and I consulted Dr. Hind on the point, and it was at his suggestion I adopted the course I did, and rejected all the observations except Mr. Tebbutt's, which gave almost uniform discordances. As he had used, in almost every instance, two stars, the errors of star places could scarcely be considered as greatly affecting the question, and I had simply to make the best I could of three solitary observations taken at as wide intervals as possible. I took the observations of Mr. Tebbutt, and from those I deduced two ellipses, whose periods differ by about 6 years—I think they were 85 and 91 years respectively. Both ellipses were deduced from the same three observations, and in each case I got errors far within the probable errors of the observations. The greatest error was in R.A., but that amounted only to 6/100ths of a second of time.

Lieut.-General Tennant: The difference between Mr. Bryant and myself is in the question, "What is a probable error?" Mr. Bryant thinks probable errors are tenths of a second. I think several seconds is not an improbable error. I am quite sure from the run of the observations, that several seconds of error were made in places. In the Windsor observations it is perfectly true that two stars were used, but they were two comparison stars, and the principal error is in observing the comet's place. The centre of gravity is not always the centre of light. In fact, I only rejected one single observation, and I thought that was a fair way of dealing with it. I would say, is it better to take three observations, and say these shall be considered as absolutely accurate; or, to take observed facts, and try to get the best results? On the whole, I am disposed to think the orbit does not differ much from a parabola.

Mr. Ranyard read a paper *On the form of the area in the heavens,* from which the meteors of Nov. 26, 1885, appeared to radiate. He said: There were a great many observers of this rich shower, and almost all agree that the meteors did not radiate from a point, but that there was a radiant area of some considerable extent. I did not observe the shower until well after its maximum, at nearly 9 o'clock; but from the 40 or 50 meteors whose paths I observed, and was able to trace back upon the heavens, I thought that the area from which they radiated was elliptical. I mentioned this to Colonel Tupman, and he told me that in the chart of paths he had laid down the radiant area was distinctly elliptical. We agreed at the time that it was probably merely a coincidence, as we did not see any reason for such an elliptic area. When Colonel Tupman described his observations at the meeting of this Society, he stated that the longer axis of this elliptic area lay north and south. This struck me as an additional coincidence with my own observation; but I did not think seriously of it till a few weeks after, when I saw a letter from Professor Young, which was published in *Nature*, in which, after describing his observations, he says that the radiant was not a point, but rather a region about 4° long, north and south, by 2° wide. I wrote to ask him further about his observation, and he replied that the radiant region was elliptical, perhaps a little larger than he had at first estimated. Recently, during a visit to the Observatory at Nice, M. Perrotin showed me a map, on which he and his assistants had laid down the courses of 60 or 70 meteors they had observed on the night of Nov. 26. I at once saw that the paths did not radiate from a point, and without telling M. Perrotin of the other observations, I asked him to draw a contour line round the area of radiation. Both he and M. Thollon, who was present, drew elliptical curves with the longer axis, north and south, or rather inclined 10° or 15° to the west of the north point. There, therefore, appears to be some very definite evidence that the paths of these meteors *did* radiate from an elongated area with its axis north and south. I have been thinking over what can be the physical cause of such an area. If the paths of the meteors were all parallel, they would appear to meet in a point. The elliptic area of radiation shows that at all events the paths within the earth's atmosphere are not parallel. It has been suggested by those who, like Sir Stawell Ball and Mr. Proctor, believe in the ejection theory of meteors, that if they were ejected in slightly different directions originally from the earth, they would not return moving in parallel paths. But if their paths outside the earth's atmosphere were not parallel, their orbits and their periods would be so very different that they would not come up to the earth again in a single swarm. I am, therefore, inclined to assume that their paths in space before they encounter the earth's atmosphere are all parallel; but that, by reason of their irregular shapes, they are deflected into slightly different directions after they enter the earth's atmosphere, usually before they have had time to become luminous, though there is some evidence that such deflection or "skidding" from their original course sometimes takes place after they become luminous. Professor Newton, Mr. B. J. Hopkins, and, I think, Colonel Tupman, have noticed meteors with curved paths. I have on two occasions noticed such curved paths myself. In order to account for the elliptic area, on the assumption that the deflection from the original path takes place within the earth's atmosphere, it is necessary to find some cause which would account for the deflection being greater in one plane than in the direction at right angles to it. This would be the case if there were some arrangement of the particles in space which caused them to set themselves with their longer axes parallel to the direction of the longer axis of the elliptic area that is north and south. I would suggest that if the particles are magnetic they would on coming up to the earth tend to arrange themselves with their longer axes parallel to the earth's magnetic axis. And in connection with this it is worth noticing that the axis of the contour drawn by M. Perrotin and M. Thollon, of which I have a tracing here, does not point due north, but some 12° or 14° to the west of north, which about corresponds to the deviation of the magnetic meridian at Nice.

The following papers were taken as read :—

J. E. Gore : *On the Orbit of* Σ 1757.

G. W. Hill : *A reply to Mr. Neison's strictures on Delaunay's method of determining the planetary perturbations of the Moon.*

Dr. H. Gyldén : *On the determination of the radius vector in the absolute orbit of the planets. Translated by G. H. Darwin.*

J. Tebbutt : *Observations of the phenomena of Jupiter's satellites, made at Windsor, New South Wales.*

R. S. Newall : *Newall's occulter.*

W. F. Denning : *Distribution of Meteor streams.*

G. Knott : *Note on the star* γ *Equulei.*

O. T. Sherman : *Answer to certain questions raised before the Royal Astronomical Society concerning the matter detailed in a paper entitled, "Bright lines in stella spectra."*

A. Marth : *Ephemeris for physical observations of Jupiter* 1887.

Royal Observatory, Greenwich : *Observations of Comet f* 1886 (Barnard).

J. G. Lohse : *Observations of Comets made at Mr. Wigglesworth's observatory with the* 15·5 *inch Cooke refractor.*

SELENOGRAPHICAL NOTES, DECEMBER, 1886.

By Thos. Gwyn Elger, F.R.A.S.

Alfraganus, Taylor, and Taylor a. The singular group of irregularly-shaped formations extending east of the crater Alfraganus is specially worthy of examination under low evening illumination, when, for instance, the terminator of the waning moon lies about 2° west of this object. The interior of Taylor being at this time in shadow its oval ring stands out conspicuously by contrast. At its north and south extremities are two small deep craters which, figuring as black projections, give to the formation a curious spindle-shaped appearance. The interior of Alfraganus is also filled with shade, tending to bring into strong relief the brilliant western segment of the wall which at this phase casts a cone of shadow nearly equal in length to the apparent diameter of the crater. Scarcely any traces of the silvery lustre associated with this object under a high sun are observable, except on the east, where there is an ill-defined glimmer. Between it and Taylor lies a large oblong plain depressed below the level of the surrounding region, its smooth dark surface being seemingly wholly devoid of detail except close to its northern end, where there is a cluster of four or five little hills, one of the number being notably larger and brighter than the others. This strange formation is of a type rarely met with, at least on so large a scale, on the lunar surface. It has no visible wall, and seems to be simply a large shallow low-lying area flanked by shelving sides. On the north side of it three short winding valleys run down to its floor from the country above, their mouths abutting on the little hills just mentioned; while on the high ground above the south-west corner there is a pair of confluent craters of unequal size. Taylor *a* is also a ring-plain of an abnormal character. It has a very lofty west wall adjoining the north-east side of Taylor, but the opposite border is so low and unimportant that even at sunset it is merely distinguished by a narrow band of shadow. On the south-east, however, it again becomes prominent, and is represented by a lofty mountain mass which towers far above all other objects in the neighbourhood. On the floor of this formation, near the inner foot of the western wall, there are two long bright ridges which, following the curvature of the border, have all the appearance of accumulations due to landslips.

On the south-west of Taylor two large irregular depressions are noteworthy, as having partially interfered with the continuity of the *glacis.* With a low power they would pass for confluent craters, but under higher magnification they appear to be deep square-shaped chasms with very rugged borders. Schmidt (sect. ii.) shows one of them only, and that as a perfectly circular crater, while both Mädler and Neison represent their true character much more accurately, though the scale of their maps is far too small to exhibit the details satisfactorily. Schmidt's representation of this particular region is less commendable than usual, as minor and unimportant features are so exaggerated that they obscure and confound others which are really prominent—a fault not chargeable to the celebrated observer himself, but rather to the mode of reproduction adopted in preparing the great chart from his drawings. Some distance west of Alfraganus he shows some curious crater-rows, which seem to be connected with the line of much larger objects of the same class south-west of Delambre, drawn as confluent and regularly diminishing in size as they proceed southwards. On August 11, 1884, I remarked a well-marked crater-row, running from north-west to south-east, so close to the western rim of Alfraganus that at 11h. 30m. it formed a tangent to the western side of the conical shadow at this time projected by the wall. It does not agree, as regards position, with the similar features shown by Schmidt, as these lie much further towards the west. In the plain north of Taylor, a row of little hillocks, running from south-east to north-west in a straight line and terminating at a bright mountain, was noted as extending to a distance of 20 miles, or more. Objects of a like description, and of greater length, are found in almost every disturbed portion of the moon, more especially in connection with clefts. It is possible that under higher powers they would be resolved into minute crater-cones, but, however this may be, they deserve more notice from selenographers than they have hitherto received.

Kempston, Beds, 20th November.

SIDEREAL NOTES FOR DECEMBER, 1886.

By J. E. Gore, F.R.A.S.

The following variable stars may be well observed in December:—

1. T Aquarii. R.A. 20h. 43m. 55s., S. 5° 34′·1 (1886·0). An interesting variable discovered by Goldschmidt in 1861. At maximum the star is 6·7 to 7·0, and at minimum it descends to about 12½ mag. The mean period is about 203·2 days, and a

maximum will be due this year about December 5. It is No. 40196 in Lalande's Catalogue, where it is rated 7½.

2. *Nova Orionis.* R.A. 5h. 49m. 7s., N. 20° 9′·2 (1887·0). This interesting object should be looked for, and if seen, its magnitude should be carefully estimated by comparison with small stars in its vicinity.

3. V Geminorum. R.A. 7h. 16m. 46s., N. 13° 18′·5 (1886·0). A variable discovered by Baxendell in 1880. It varies from about 8·7 mag. to about 13 mag. The mean period is about 276 days, and a maximum will be due about December 6.

Minima of Algol may be observed on December 6 about 11h. 15m. p.m.; December 9, 8h. 5m. p.m.; and December 29, at 9h. 47m. p.m.

The following "suspected" variables are well placed for observation in December:—

1. Lalande 7172 Tauri. R.A. 3h. 47m. 5s., N. 7° 26′·1 (1886·0) 8½ Lalande; 9 Harding; 7·8 D.M. The Cordoba estimates vary from 6·8 to 7·9. My observations (1883—1885) record the star as 7·8 to 8·2.

2. Birmingham 85 Tauri. R.A. 4h. 44m. 22s., N. 28° 19′·8 (1886·0). 9 Bessel; 8·1 Argelander. Birmingham's estimates of magnitude (1872—1876) vary from 7 to 10 mag.; 10 mag. Webb, January 12, 1874, and 9 mag. March 7. I found it below 8 mag., November 26, 1878.

3. Birmingham 96 Orionis. R.A. 4h. 59m. 12s., N. 1° 0′·7 (1880·0). 6½ and 7 Lalande (9581–2); 6 and 7 Birmingham; 8 Webb; 6·6 at Cordoba. On March 9, 1885, I estimated it 7·0; November 30, 1885, 6·8; and March 3, 1886, 7·1. The star is evidently variable to some extent. Dunér finds a splendid spectrum of the type III. *b*. Vogel calls its colour "Gelblich roth," but Dunér finds it "presque rouge absolu."

4. Lalande 9667 Eridani. R.A. 5h. 1m. 41s., S. 8° 48′·7 (1880·0). 8 mag. Lalande; 7 Bessel. According to Dr. Gould it is generally below 7½ mag.; but it was once observed by Mr. Rock as 6·9 (*U. A.*, p. 273). I found it below 7½ mag. on the following dates:—March 3, 1884; January 14 and February 14, 1885.

5. Birmingham II. 16 Leporis. R.A. 5h. 6m. 10s., S. 12° 2′·0 (1880·0). This red star, which lies closely *preceding* ι Leporis, has been variously estimated by different observers (1755—1886) from 5·0 to 7·7 mag. It is not given by Argelander or Heis. Flammarion says: "Elle est certainement variable." Both D'Arrest and Dunér find a magnificent spectrum of Secchi's third type, and Dunér remarks, "Le spectre est un des plus superbes au ciel." It was measured 5·86 at Harvard,

and I have always found it about this magnitude. It may, however, be an irregular variable with a long period.

6. Bond 822 Orionis. R.A. 5h. 30m. 15s., S. 5° 33'·1 (1886·0). A small star in the Orion Nebula, found to be variable by Schmidt in 1878, and by Common (by means of photography) in 1883. At maximum it seems to be about 9·7 mag., and at minimum 12·8. There seems to be no doubt about the variability of this star, but the period has not yet been determined.

*RECENT RESEARCHES ON THE DISTANCES OF THE FIXED STARS, &c.**

There has ever been a desire to burst aside the constraints imposed upon our research by the distance of space; to pass from the study of the planets of our solar system to that of the suns and galaxies that surround us; to determine the position and relative importance of our own system in the scheme of the universe, and the whence we have come, and the whither we are drifting through the realms of space.

The galaxy or Milky Way—what is it? Is our sun one of its members? What is the shape of that galaxy? What are its dimensions? What is the position of our sun in it?

The star-clusters—what are they? Are these clusters galaxies? Have these suns real dimensions, comparable with those of our sun? and is it distance alone that renders their light and dimensions so insignificant to the naked eye? Or are the real dimensions of the clusters small, as compared with our galaxy? Are their component suns but the fragments of some great sun that has been shattered by forces unknown to us, or have they originated from chaotic matter, which, instead of forming one great whirlpool, and condensing by vortex action into one great sun, has been thrown into numerous minor vortices, and so become rolled up into numerous small suns.

The nebulæ—what are they? Are they, too, condensing into clusters of stars? or will their ghost-like forms remain for ever unchanged amongst the stars? or do they play some part in the scheme of nature of which we have as yet no conception?

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* *Recent Researches on the Distances of the Fixed Stars, and some future Problems in Sidereal Astronomy.* By David Gill, Esq., LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope. A lecture delivered at the Royal Institution of Great Britain, May 23, 1884. (Here slightly abridged, and with the omission of the diagrams. From the Proceedings of the Royal Institution. Vol. XI. Part I. No. 78. Feb., 1885.)

These and many others are the questions which press on the ardent mind that contemplates the subject; and there arises the intense desire to answer such questions, and, where facts are wanting, to supply facts by fancy. The history of deep and profound thought in some of these subjects goes back through 2,000 years, but the history of real progress is but as of yesterday. The foundations of sidereal astronomy may be said to have begun with the art of accurate observation. Bradley's meridian observations at Greenwich about 1750, his previous discovery of the aberration of light in 1727, and Herschel's discovery of the binary nature of double stars, his surveys of the heavens, and his catalogues of double stars—these are solid facts, facts that have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They point to us the lesson that "art is long and life is short," that human knowledge, in the slow developing phenomena of sidereal astronomy, must be content to progress by the accumulating labours of successive generations of men, that progress will be measured, for generations yet to come, more by the amount of honest, well directed, and systematically discussed observation, than by the most brilliant speculation, and that in observation, concentrated systematic effort on a special thoughtfully-selected problem, will be of more avail than the most brilliant but disconnected work.

I hope that no one present thinks, from what I have said, that I undervalue the imaginative, fervent mind that longs for the truth, and whose fancy delights to speculate on these great subjects. On the contrary, I think, and I believe that without that fervent mind, without that longing for the truth, no man is fitted for the work required of him in such a field—for it is such a mind, and such desires, that alone can sweeten the long watches of the night, and transform such work from drudgery into a noble labour of love.

It is for like reasons that I ask you to leave with me the captivating realms of fancy this evening, and to enter the more substantial realms of fact.

We suppose ourselves, then, face to face with all the problems of sidereal astronomy to which I have hastily referred—the human mind is lost in speculation, and we are anxious to establish a solid groundwork of fact.

Now, what in such circumstances would be the instinct of the scientific mind?

The answer is unquestionable, viz., to measure; and no sooner were astronomical instruments made of reasonable exactness than astronomers did begin to measure, and to ask, are the distances of the fixed stars measurable?

I should like to have given a short history of the early attempts of astronomers to measure the distance of a fixed star. But I must come at once to the time when the long-baffled labours of astronomers began to be crowned with success.

Before I begin, it will save both time and circumlocution if I define a word that we must frequently use—viz., the word "parallax."

It may be defined as the change in the apparent place of a star produced by viewing it from a point other than that of reference. [The lecturer here gave some practical illustrations of parallax.] Our point of reference for stars is the sun, and as we view the stars, now from one side of the sun, and six months afterwards from a point on the opposite side of the sun—that is, from two points, 186 millions of miles apart—we might expect to find a considerable change in their apparent places. But previous to 1832 astronomers could not discover, with any certainty, that such changes were sensible; or, putting it in another way, the stars were so distant that the diameter of the earth's orbit, viewed from the nearest star, subtended a smaller angle than their instruments could measure. Bradley felt sure that if the star γ Draconis were so near that its parallax amounted to 1″ of arc, he would have detected it—that is, if the earth's orbit, viewed from γ Draconis measured 2″ in diameter (or as big as a globe, one foot in diameter would look, if viewed at 40 miles distant) he would have detected it. But the real distances of the stars were greater than that.

The time at last arrived when the two great masters of modern practical astronomy, Bessel and Struve, were preparing by elaborate experiment and study, for the researches which led to ultimate success. After vain attempts to obtain conclusive results by endeavours to determine the apparant changes in the absolute direction of a star, at different seasons of the year, both astronomers had recourse to a method which, originally proposed by Galileo in 1632, was carried out first on a large scale by Sir William Herschel. I shall refer in the first place to the researches of the great Russian astronomer Struve.

Astronomers had sufficiently demonstrated that the distances of the stars were very great, and it was reasonable to argue that as a rule the brighter stars would be those nearest to us. If, therefore, two stars are apparently near each other—the one bright, the other faint—the chances are, that in reality they are far apart, though acccidentally nearly in a line. [The lecturer then exhibited in a diagram how two stars would appear near each other viewed from one side of the earth's orbit, but not so near each other viewed from the opposite side of the earth's orbit.] Struve selected the bright star Vega (*α* Lyræ). From its brilliancy he considered it probably one of our nearest neighbours amongst the stars, and a faint star apparently near it seemed to afford a suitable representative of the really distant star. Struve was careful to ascertain that this comparison star was not physically connected with *α* Lyræ, and he was able to prove this from the fact that whilst *α* Lyræ has a small annual motion relative to all neighbouring stars, this motion is not shared by the faint comparison star. Struve was provided with a telescope driven by clockwork to follow the diurnal motion of a star, and thus the hands of the observer were free to make the necessary measures. These were accomplished by an instrument such as I hold in my hands, applied to the telescope. This micrometer contains the parallel spider-webs, each attached to a slide, one slide being moved by one screw, the other by the other screw. The screws are provided with drum-heads, divided into 100 parts. One web was placed on the image of *α* Lyræ, the other upon that of the faint comparison star, and the angle between the stars was thus read off in turns of the number of revolutions and decimals of a revolution of the screws. A number of such observations was made on each night, and the result for each night depended on the mean of the numerous observations made each night.

By observations on ninety-six nights, between November, 1835, and August, 1838, he showed that the distance between *α* Lyræ and the faint comparison star changed systematically with a regular annual period, and that the maxima and minima of those distances corresponded with the times of the years at which these maxima and minima should occur if the brighter star were really much nearer than the fainter one.

Assuming that the fainter star is at a practically immeasurable distance, Struve showed that *α* Lyræ had a parallax that amounted to about a quarter of a second of arc, which is equivalent to the statement that a globe whose diameter is equal to that of the earth's orbit—that is, to 186 millions of miles—would, at the distance of *α* Lyræ, present an apparent diameter of half a second of arc. If you wish to realize this angle, place a globe one foot in diameter at a distance of 80 miles; or look at a coin half the diameter of a silver threepenny piece at a distance of one mile from the eye, and try to measure it.

The great German astronomer, Bessel, was simultaneously engaged in like work at Königsberg. He selected as the object of his researches a very remarkable double star, 61 Cygni. This star had already been the subject of similar researches on his part with much inferior means. He now attacked the problem with the splendid heliometer which had been made for him by Fraunhofer for the purpose. The principle of this instrument I shall presently explain. His reasons for choosing 61 Cygni were that the two components of this star, though not remarkable for brightness—they are just visible to the naked eye—yet have this peculiarity, that they have a remarkably large proper motion, the largest then known, though now known to be surpassed by that of two other stars which I shall afterwards mention. The components of 61 Cygni have an apparent angular motion relative to other stars of more than five seconds of arc per annum.

Struve had argued that if the stars were on the average of similar brightness, those stars which were brightest would probably be those nearest to us, and Bessel, in like manner, argued that if the absolute motions of the stars were similar on the average, those motions which appeared the largest belonged to stars which, on the average, were nearest to us, just as the motion of a snail could be easily watched at the distance of two or three feet from the eye, but could not be detected except after a long interval if the animal were a great many yards distant.

Bessel employed two faint comparison stars at right angles to each other with respect to 61 Cygni, and he made two separate series of observations, the first extending from August, 1837, to October, 1838; the second from October, 1838, to March, 1840.

Both series confirm each other, and the results deduced separately from the measures of the two comparison stars also agree within very narrow limits. From all the observations combined Bessel found the parallax of 61 Cygni to be 35/100 of a second—a quantity which has been shown by the modern researches of Prof. Anwers and Dr. Ball to be more nearly half a second of arc; thus at 61 Cygni the diameter of the earth's orbit round the sun would appear of the same size as a globe a foot in diameter viewed at 40 miles distance, or of a silver threepenny piece a mile off. But whilst these great masters of astronomy, Struve and Bessel, had been exhausting the resources of their skill in observations, and that of the astronomical workshops of Europe in supplying them with the most refined instruments, a quiet and earnest man had been at work at the Cape of Good Hope, and without knowing it at the time, had *really made the* FIRST *observations* which afforded strong presumptive evidence of the existence of the parallax of any fixed star.

REVIEWS.

The Phainomena, or Heavenly Display of Aràtos. Done into English verse by Robert Brown, Jun., F.S.A. London: Longmans, Green & Co. 1885. pp. xiv. 102. Price 10s. 6d.

The author says: "In the present work I place before the reader in an English form the famous poem of Aratos, which has supplied our modern world with the now familiar ancient constellations. The *Phainomena* was one of the most popular productions of antiquity. . . The statements of Aratos are frequently being incorrectly quoted, and it seemed desirable to place them plainly on record, and to accompany them with a short introduction and some brief illustrative notes. . . . One of the chief points of interest in the enquiry is the connection, ever becoming clearer, between Hellenic and Euphratean astronomy, a subject necessarily almost entirely unknown to earlier writers." The work has 68 illustrations, some from Babylonian stones, and other ancient relics, and grotesque drawings from old German MSS., the Farnese globe, the MS. of Cicero's Aratos, and Montfaucon have also contributed some. There is likewise a map of the principal stars near the equator for the equinox B.C. 2084.

Aratos was a native of Foli, in Cilicia, or (according to one writer) of Tarsus. He lived about B.C. 270. This poem is believed to be in a great measure a verification or close imitation of a prose work with the same title by Eudoxus, a century before. It is from verse 5 that the Apostle Paul (Acts xvii. 28) made his well-known quotations, though some think the stoic Cleanthus, B.C. 300, may be also alluded to, who in his hymn to Zeus has nearly the same words. We have no doubt that Mr. Brown's translation is faithful, but we have not a copy of the original at hand to compare it with. He says he has nearly always followed the text of Bekker, the 732 lines of Greek having been rendered into the same number of English lines. In most cases the number of a verse applies to the original as well as to the translation. The author is evidently a man of extensive reading in both ancient and modern works, and those who give their attention to the mythology of the stars will be interested in his production. The volume is dedicated to Mr. Gladstone.

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1886.	°	′		°	′		°	′
Dec. 1	+19	35M.	Dec. 11	+77	59 E.	Dec. 21	—43	36 E.
2	7	25	12	65	49	22	55	46
3	4	45	13	53	39	23	67	55
4	—16	54	14	41	30	24	80	5
5	29	4	15	29	20	25	+87	46
6	41	13	16	17	11	26	75	36
7	53	23	17	5	1	27	63	27
8	65	33	18	— 7	8	28	51	18
9	77	42	19	19	17	29	39	9
10	89	52	20	31	26	30	26	59
						31	14	49

Moon farthest from Earth, Dec. 3, 3h.
,, nearest to ,, ,, 15, 12h.
,, farthest from ,, ,, 31, 0h.

ASTRONOMICAL OCCURRENCES FOR DECEMBER, 1886.

DATE.		Principal Occurrences.		Jupiter's Satellites		Meridian Passage.
		h. m.			h. m. s.	h. m. Aldebaran.
Wed	1		Sidereal Time at Mean Noon 16h. 41m. 3·81s.	1st Sh. I. 1st Tr. I. 1st Sh. E.	17 11 18 0 19 24	11 46·4
Thur	2	17	Superior conjunction of Venus and Sun Sun's Meridian Passage 10m. 21·22s. before Mean Noon	1st Oc. R.	17 32	11 42·5
Fri	3	2 25 5 8 6 30 0	☽ Moon's First Quarter Occultation of h^1 Aquarii (5½) Reappearance of ditto Inferior conjunction of Mercury and Sun	2nd Oc. R.	19 33	11 38·6
Sat	4					11 34·6
Sun	5	4 52	Near approach of 14 Ceti (6½)			11 30·7
Mon	6					11 26·7
Tues	7			3rd Ec. R. 3rd Oc. D. 3rd Oc. R.	15 37 10 17 7 19 21	11 22·8
Wed	8			1st Sh. I.	19 4	11 18·8
Thur	9	17 52	Occultation of 48 Tauri (6) Saturn's Ring: Major axis=45″·69 Minor axis=17″·85	1st Ec. D. 1st Oc. R.	16 23 47 19 31	11 14·9
Fri	10	21 30 10 29 11 44	○ Full Moon Occultation of B.A.C. 1526 (6) Reappearance of ditto	1st Sh. E. 1st Tr. E. 2nd Ec. D.	15 45 16 41 17 48 40	11 11·0
Sat	11					11 7·1
Sun	12			2nd Sh. E. 2nd Tr. E.	15 22 17 15	11 3·2
Mon	13	13 42 14 46 18 54 5	Occultation of 3 Cancri (6) Reappearance of ditto Near approach of B.A.C. 2731 (6½) Conjunction of Moon and Saturn 2° 59′ N.			10 59·2
Tues	14	9 26 10 9	Occultation of 54 Cancri (6½) Reappearance of ditto	3rd Ec. D. 3rd Ec. R.	17 29 7 19 34 25	10 55·3
Wed	15		Illuminated portion of disc of Venus=0·999 Illuminated portion of disc of Mars=0·969			10 51·4

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Thur	16		Sidereal Time at Mean Noon 17h. 44m. 8·72s.	**1**st Ec. D.	18 17 21	Alde-baran. 10 47·4
Fri	17	18 39	☾ Moon's Last Quarter Sun's Meridian Passage 3m. 37·76s. before Mean Noon	**1**st Sh. I. **1**st Tr. I. **1**st Sh. E. **1**st Tr. E.	15 25 16 26 17 38 18 39	10 43·5
Sat	18	13 50 14 34 14 53 15 21	Occultation of γ^1 Virginis (2½) Reappearance of ditto Occultation of B.A.C. 4277 (6) Reappearance of ditto	**1**st Oc. R.	15 58	10 39·6
Sun	19			**2**nd Sh. I. **2**nd Tr. I. **2**nd Sh. E. **2**nd Tr. E.	15 17 17 22 17 57 19 59	10 35·6
Mon	20	3	Conjunction of Moon and Jupiter 3° 24′ S.			10 31·7
Tues	21					10 27·8
Wed	22					10 23·8
Thur	23	2	Conjunction of Moon and Mercury 3° 5′ S.			10 19·9
Fri	24	21 54	● New Moon	**1**st Sh. I. **1**st Tr. I.	17 18 18 23	10 15·9
Sat	25	10	Conjunction of Moon and Venus 4° 39′ S.	**1**st Ec. D. **3**rd Tr. I. **3**rd Tr. E. **1**st Oc. R.	14 39 11 15 38 17 40 17 56	10 12·0
Sun	26			**1**st Tr. E. **2**nd Sh. E. **2**nd Tr. I.	15 5 17 53 20 5	10 8·1
Mon	27	7	Conjunction of Moon and Mars 3° 29′ S.			10 4·2
Tues	28	6 31 7 34	Occultation of 29 Capricorni (6) Reappearance of ditto	**2**nd Oc. R.	16 59	10 0·2
Wed	29		Saturn's Ring: Major axis=46″·42 Minor axis=18″·45			9 56·3
Thur	30					9 52·4
Fri	31			**1**st Sh. I.	19 11	9 48·4

THE PLANETS FOR DECEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	16 50 54	S.21 50½	9″·8	0 9·8
	9th	16 10 40	S.18 15	8″·8	22 53·7
	17th	16 15 7	S.18 40½	7″·2	22 27·3
	25th	16 47 2	S.20 50	6″·0	22 28·3
Venus ...	1st	16 33 57	S.21 50	9″·8	23 49·0
	9th	17 11 44	S.23 7	9″·8	* *
	17th	17 55 35	S.23 55½	9″·8	0 11·4
	25th	18 39 41	S.23 53	9″·8	0 23·9
Jupiter ...	20th	13 55 59	S.10 37	31″·2	19 56·7
	28th	14 0 34	S.11 0½	32″·0	19 29·9
Saturn ...	1st	7 34 3	N.21 27	18″·0	14 50·5
	9th	7 32 10	N.21 31½	18″·2	14 17·1
	17th	7 29 56	N.21 27½	18″·4	13 43·5
	25th	7 27 25	N.21 43½	18″·4	13 9·6
Neptune ...	3rd	3 36 27	N.17 32½	...	10 45·7
	23rd	3 34 26	N.17 26½	...	9 25·1

Mercury rises on the 3rd, a few minutes before the sun, the interval rapidly increasing.

Venus sets just after the sun at the beginning of the month, the interval increasing.

Jupiter rises three hours and a half after midnight on the 1st, the interval decreasing.

FINIS.

VALETE.

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THE ASTRONOMICAL REGISTER.

DECEMBER, 1886.

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